THE PRINCIPLES

OF

EMPIRICAL OR INDUCTIVE LOGIC

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TORONTO

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OF

EMPIRICAL OR INDUCTIVE LOGIC

BY

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MACMILLAN AND CO., LIMITED
ST. MARTIN'S STREET, LONDON
1907

First Edition 1889.
Second Edition 1907.

PREFACE TO FIRST EDITION.

THIS work contains the substance of Lectures delivered during a number of years in Cambridge; at first to members of my own College, and afterwards to students generally in the University. Although the main outlines were sketched long ago, and a large portion of the materials had been delivered for several years in nearly the form now presented, the chapters here offered to the reader have been throughout written out afresh for the present occasion.

As many readers will probably perceive, the main original guiding influence with me,—as with most of those of the middle generation, and especially with most of those who approached Logic with a previous mathematical or scientific training,—was that of Mill. But, as they may also perceive, this influence has subsequently generated the relation of criticism and divergence quite as much as that of acceptance; though I still continue to regard the general attitude towards phenomena, which Mill took up as a logician, to be the soundest and most useful for scientific study.

This attitude of the scientific logician, as I conceive and interpret it, has been so fully explained in the introductory chapter, that I need only say that it is based upon that fundamental Duality in accordance with which it becomes the function of the logician to reduce to order, to interpret, and to forecast the complex of external objects which we call the phenomenal world. Whatever there may be that is at all

distinctive in the following treatise,—for instance in the explanation of Hypotheticals, and in the discussion of the relation of Art or Conduct to Science,—follows I think from a more thoroughgoing adherence to this view than is customary amongst writers on Logic. By the introduction of the term *Empirical* into the title, I wish to emphasize my belief that no ultimate objective certainty, such as Mill for instance seemed to attribute to the results of Induction, is attainable by any exercise of the human reason.

It will be seen that I have made comparatively slight reference to other, and especially to contemporary writers. In my former works the opposite course was adopted, but for special reasons. As regards Probability, much had recently been produced which was only accessible in scattered numbers of various scientific journals, and every student knows how apt these are to be overlooked unless special attention be directed to them. And as regards Symbolic Logic, the history and literature of the subject had been so entirely neglected that even the names of most previous writers on the subject were quite unknown to their modern successors. In such a province, however, as that of Material or Inductive Logic the case is very different. Here every writer has long had almost exactly the same materials before him, so that the only novelty at which he can aim must be confined to such modification of the old problems as can be effected by regrouping the familiar conceptions, and by careful appeal to the recently accepted methods and results of Physical and Natural Science. This being so, comment upon the work of others would principally take the form of criticism, and this I have wished to avoid as much as possible, in order to keep down the size of the present edition, and, if another should be called for, to reduce the bulk of the always inevitable reply and explanation which is there demanded. In fact, both from my own tastes and from the process by which these chapters were originally com-

piled, it has been my wish that the work should be as much as possible constructive rather than critical. Writing mainly for English academic students I have made my references prominently to works which such students are likely to have read or to find at hand. At the same time no effort has been spared, as I hope will be perceptible to the competent reader, to become acquainted with all the best recent contributions to the subject, whether these refer to the methods and results as treated from the stand-point of pure science, or to the general principles as treated by the more professional logician. Among recent works of the latter kind, which, as covering approximately the same general field of Objective or Material Logic and Methodology, had most claim on my attention, may be mentioned the treatises of Sigwart and of Wundt.

I have to express my sincere acknowledgement to Mr W. E. Johnson, of King's College, for aiding me in the revision of the proof-sheets. Many corrections and suggestions are due to his accurate judgment and thorough knowledge of the subject.

J. VENN.

CAIUS COLLEGE, CAMBRIDGE,
March, 1889.

PREFACE TO SECOND EDITION.

THERE is no occasion to offer any reason or excuse for the issue of a second edition of a work when the exhaustion of the first edition still leaves a certain demand.

As the reader will see, in the arrangement originally adopted it was throughout presupposed that Mill's Logic was familiar to my hearers, and that they had the work in their hands. Mill, in fact, dominated the thought and study of intelligent students to an extent which many will find it hard to realize at the present day. And they mostly absorbed his teaching at first hand. Had I been still in active work I should of course have seriously considered the desirability of re-writing and re-casting the whole scheme. This is out of the question for one who has now for some years ceased either to lecture or to examine. However, any such change seems the less necessary inasmuch as I am assured by those who are now more in contact with the modern student than I am that the former familiarity with the doctrines of Mill may be still taken for granted to a considerable extent, though doubtless it is now obtained, more than was formerly the case, through derivative channels.

Though thus leaving the main outlines unaltered I have done what I could to improve the work, and to try to bring it up to date. What help could be gained from the reviewer or the chance critic has been welcomed. A number of paragraphs have been altered, others have been re-written, and many hundreds of minor alterations, additions and corrections inserted.

Amongst the friends or acquaintances who have given me help I must mention Mr A. T. Shearman, author of *Development of Symbolic Logic*, who has aided me by a number of useful suggestions.

J. VENN.

CAIUS COLLEGE, CAMBRIDGE,
August, 1907.

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CHAPTER I.

THE FOUNDATIONS OF LOGIC:—THE UNIVERSE AS THE MATERIAL LOGICIAN REGARDS IT.

SINCE Logic, as conceived and expounded in this work, is not an ultimate science, in the sense of being concerned directly with really first principles of any kind, we find ourselves obliged, on a general survey of our province, to take for granted that a great deal has already been accomplished or decided for us in various directions. In other words, we have to demand a variety of postulates, drawn partly from the region of Metaphysics, partly from those of Psychology, Physical Science, Grammar, and so forth. Some of these postulates will be readily accepted by every one: others will be admitted by those who have had any philosophic training: some, I take it, have hardly yet been duly appreciated or even recognized. This being so, it would seem convenient that the more important of these postulates should be prominently and definitely stated at the outset. For one reason or another, however, such a course seems seldom adopted, and the result has been disadvantageous in more ways than one. This neglect to state the postulates has, for instance, brought down upon the logician charges of inconsistency and shortsightedness, which might as fairly be brought against the representatives of most other sciences, but which seemed damaging when he had to meet them alone. It has also tended, as I shall trust to show in the sequel, to encourage mistaken views as to the functions and province of the science; whilst the general objections to such an omission, on the grounds of order and method, are too obvious to need enforcement.

The reader need hardly be reminded that, in such a preliminary statement of assumptions, we cannot fairly be called upon fully to justify them. They would not be assumptions or postulates, if we were to undertake to do this. We ought,

however, to endeavour to explain their nature as clearly as possible, and to give some kind of indication of the grounds for resorting to them, and of those problems of our science in respect of which difficulties will be removed by their acceptance.

I. When we claim, as the first of such postulates, the existence, and the general recognition of external 'objects', in the widest sense of the word, it may seem to the reader unfamiliar with philosophy as if we were not making any assumption worth mentioning. He would be inclined to take it for granted that these objects exist; and that consequently all that we have to do, so far as our logical premises are concerned, is just to open our eyes and other organs of sense, and perceive what is before us.

The fact is however that certainly one stage, and possibly two, must have been passed through, before the simple recommendation to observe the objects before us can be carried out. If accepted, they are both stages of the utmost importance; and historically,—that is, in the gradual development of the human race,—each may have demanded an enormous time for its accomplishment.

(1) In the first place then, in the opinion of many philosophers, the primary stage of recognizing that 'objects' are outside us at all, has had somehow to be reached. They maintain that the only indubitable data of consciousness consist in our own subjective impressions, and that everything beyond these is inference, instinctive suggestion, illusion, or convention of language, according as we regard it or like to express it. This primary postulate seems to belong so completely to the domain of Metaphysics in contradistinction from that of Logic, that we should hardly have thought it necessary to state it here, but for the fact that much more space than will be required thus to notice it is not unfrequently wasted indirectly, by the introduction of discussions or quibbles arising almost entirely out of the neglect to notice it. Refer, for instance, to Mill's Logic. When he is discussing his arrangement of the Categories, he throws into two separate subdivisions, respectively, our own simple sensations and the external objects which are supposed to give rise to these. But then he almost immediately proceeds to disclose the fact that in his own opinion all external objects are, in the ultimate analysis, nothing else than states of con-

sciousness; and so the distinction seems to be broken down again. Not only is this a cause of perplexity to one beginner after another, but his arrangement has been made a serious ground of complaint by more than one critic. He has been told that, as regards the Categories, he has no business to distinguish 'bodies' from 'sensations'; and that, in the corresponding question as regards the Interpretation of Propositions, he has equally no business to distinguish between propositions which deal with 'facts' and those which deal with 'ideas'.

But there is really no necessary inconsistency here, if we only bear in mind that Logic is not an ultimate science, but stands, so to say, upon a plane at the same depth of philosophic analysis as do the various physical sciences. The existence of an external world, in fact, is just one of those questions which a man must be left to settle with his metaphysician, but which he has no reason to introduce in the case of any quarrel between himself and his logician. He cannot utter any of the precise statements of logic, or any of the looser ones of common life: he cannot claim to be right upon any subject of discussion, he cannot be shown to be wrong: he cannot even ask a question which goes outside his own private feelings: without admitting all that we require for our present purposes. 'Things', if they were ever, at any very early epoch of our mental development, consciously constituted by our sensations or groups of sensations, must have already 'fallen back' from us, out of their simply subjective condition. By direct intuition, by passive association, by some kind of active mental construction, or by one or other of the various means which philosophers have suggested, we must have come to contemplate the world and to reason about it, as if it were mainly composed of things or phenomena external to our own minds. Or, to take a simple concrete example; when I utter the statement 'The sun is hot': it is not relevant in Logic, however suitable in Metaphysics, for any one to interpose the objection that the sun *is* nothing but sensations of light, heat, &c.; and that we are therefore only connecting together two kinds of sensation, rather than an object and a sensation. Such criticism is simply irrelevant in Logic, nearly as much so as it would be in Physics or in Zoology. We must postulate, at our starting-point, objects *and*

our sensations, not simply two sets of sensations of which one is more complex than the other.

Our first postulate therefore is simply the resolution to start with a *duality* of existences;—our sensations and ideas on the one hand, and the materials or objective data which constitute the world of phenomena on the other. As already remarked, this postulate is only required as against certain metaphysicians, or as against critics who raise objections on behalf of the metaphysicians which these would not always raise for themselves. The bulk of ordinary thinkers and observers, whether scientific or not, will freely grant the assumption, and probably only wonder at the necessity of any such formal statement of it.

(2) But very much more than this is demanded. Detached fragments of externality, however completely we may have thus projected them outside our own personality, will not suffice to produce even an irregular and chaotic world, for they will not avail to constitute the separate 'objects', however fragmentary and disorderly these may be, with which a chaos must be conceived to be occupied. A good deal of positive constructive effort is demanded in order to bring into being even

> "a dark
> Illimitable ocean, without bound,
> Without dimension; where length, breadth, and height,
> And time, and place, are lost; where eldest Night
> And Chaos, ancestors of Nature, hold
> Eternal anarchy, amidst the noise
> Of endless wars; and by confusion stand."

In order to constitute these warring objects a very considerable amount of grouping, with the correlative process of distinction and separation, must have been already accomplished. This step, unlike the one above, is a step about the reality and necessity of which there can be no doubt, and it is one which we can readily conceive ourselves as actually engaged in taking. The human race, as a whole, must certainly have gone through this continuously interchanging process of Analysis and Synthesis, and is in fact still perpetually, though slowly, carrying it on at the present time. And each individual of the race concurs in the carrying out of this process, to a greater or less extent, according to the independence of his mind, and to the keenness of his observing and discriminating faculties. Of course most

of us, and at most times, are almost entirely passive here. We find the work pretty effectively done to hand for us at our birth; the instrument by which it is thus accomplished and perpetuated for us being, it need not be said, the language we inherit from our predecessors.

Although, however, the work is mostly found ready done for us at our birth, we can easily put ourselves in imagination into the position of having had to originate it for ourselves, and it is only by such a supposition that we can realize its magnitude and importance. By devices familiar enough in psychological discussions we can picture to ourselves a man with mature faculties but with nascent experience;—one somewhat in the state which Buffon has strikingly illustrated in a curious digression in his Natural History, where he has given a brief autobiographic sketch of our first parent's supposed experience on his first day and night in Paradise. This experience however mostly refers to a later stage than that to which we might go back. It refers to such doubts as might be entertained as to whether the sun will continue to rise and to set, and to warm and enlighten the earth. These are most important questions, and their due consideration will fully occupy us in the course of the next two or three chapters. What must be insisted upon at the present moment is rather the process by which we have come definitely to formulate such doubts. Before we can ask whether the sun will rise and set again, we must have reached the point of recognition,—*of perception in fact*,—of such objects as sun, earth, and our bodies, and of such processes as lighting and warming.

Whatever may be said here under this head will be familiar enough to the reader who knows something of Psychology, but it may be convenient to add a little explanation for the use of the beginner, and to point out the peculiar logical significance of the process in question. Even were we to grant all that the Natural Realist would claim, viz. that the separate elements of the various phenomena of nature have always stood out distinct from our own personality, clearly detached from the first moment as they seem now, yet there would not consequently be to us any 'sun' or 'earth' to form the subjects of even our most doubtful and interrogative propositions, nor any 'light' and 'warmth' to form their predicates. Select what object we

please,—the most apparently simple in itself, and the most definitely parted off from others that we can discover,—yet we shall find ourselves constrained to admit that a considerable mental process had been passed through before that object could be recognized as being an object, that is as possessing some degree of unity and as requiring to be distinguished from other such unities. Take one of the most obvious instances of a persistent unity, say the sun. This has to be identified day after day, in the East and in the West, behind cloud or haze and glaring down upon our heads. But identification of an object under varying circumstances means nothing else than the capacity of holding together, in a mental synthesis, certain elements which in nature are often and widely separated; and also of separating from each other elements which from time to time are actually found to be conjoined. Unless this process had been adequately carried out we might be dazzled with light, or be scorched with heat, and others might express the actual facts and their relation to us, in the form of propositions; but *we* could not be said to see or feel the sun in any other sense than that in which one might declare of a dog, for instance, that he perceives 'the British character'. The ultimate constituent elements of the perception and of the corresponding assertion are presented to the sensitive agent, but unless he has himself grouped them aright, he cannot be said to perceive the object.

Reference has just been made to the instance of an animal and its perceptive powers. It is worth dwelling a little further upon this illustration, in some familiar application. I do not ask, then, whether the dog believes that the rainbow is a sign of good or bad weather, but raise the previous question, whether he can be said to see the rainbow at all. That every detail of colour and of form is painted upon his retina, as upon ours, when the eye is turned in the right direction, is beyond all reasonable doubt. But such an admission only carries us a very little way. The rainbow, regarded as a visible object, consists of a group of colours arranged in a certain shape. The outline of the red circle, say, has to be recognized and traced, though it may actually be intermittent in the instance before us, and its colour has to be distinguished from any other patches of red which there may happen to be in the field of view. So with the

other colours; and the group of all together has to be united into a somewhat artificial mental whole. What reason have we to suppose that the dog goes through all this analysis and synthesis in a matter which cannot possess the slightest interest for him? So much however are we in the habit of regarding what we call 'objects' as being in a way marked out by nature, always and for all beings, that to raise a doubt whether the dog really sees the rainbow would be taken by many persons as indicating a disbelief in his actual optical powers. One might however almost as reasonably expect him to see, say, the 'Progress of Democracy' in the place where he lives, of which course of events the ultimate constituent sensible elements are accessible to his observation precisely as they are to ours.

If the reader feels any difficulty in accepting the above suggestion he need only pause to consider what range of difference exists, as a matter of present fact, between one person and another in respect of what they recognize. The following simple example will serve our purpose. Every one who has once had the appearance brought under his notice, is familiar with the curious and regular curvilinear patterns which present themselves when the wheels of a rapidly passing carriage are looked at a little sideways, so that the centres of the two wheels are not quite in the same line of vision[1]. In certain respects these patterns have a remote analogy to the rainbow,—e.g. in that they arise out of a certain fixity of instantaneous relation to us of elements which are themselves in rapid motion; in that it may be maintained that no two persons can be said to see the same object strictly: and so on;—and they have exactly the same title, neither more nor less, to be regarded as objective entities, viz. as 'things'. Inasmuch as they are of no importance to us, and have not, like the rainbow, acquired a name, very few persons ever notice them. Once pointed out, they readily force themselves upon the view; but though every day

[1] They arise from the fact that certain portions of the overlapping spokes, at any given moment, lie longer in the same line of vision, and consequently cause a more durable after image. The determination of the exact pattern is of course a mere geometrical problem. The modern bicycle, with its polished steel spokes arranged in two approximately parallel circles on each wheel, shows the pattern much more vividly; so that in the case of these machines we do not find the striking contrast between one observer being unable *not* to see what ninety-nine others under similar circumstances are unable *to* see.

in London there must be tens of thousands of persons who have all the requisite impressions made on the retina, experience shows that an extremely small proportion of mankind have ever seen them.

The logical bearings of the above state of things are manifold, and will have to be discussed in due place as they present themselves. The most obvious of these bearings is that on the nature of the Categories,—if we interpret these, with Mill, as being the most fundamental divisions and classifications of all Nameable Things—for we cannot satisfactorily establish the range of these unless we have realized the exceedingly complex and artificial character of many of them. Similarly with the nature of propositions; the relation of subject and predicate; the distinction between existential propositions and those which involve a distinct copula; nay also,—as I shall hope to show,—much of the difference between the hypothetical and categorical forms of statement. We shall never treat these satisfactorily unless we realize that Logic must take it for granted, as one of its postulates, that an enormous amount of this object-manufacture has been already got through and that the result lies ready to hand for further use.

One obvious objection may be noticed here. It may be urged that, so far from this process of analysis and synthesis of elements—by which, as we have just pointed out, the various objects which collectively form the vast complex of our logical world, have been put together,—being presupposed by Logic, such processes are really the principal subject-matter of the science. For what else, it may be urged in many cases, are affirmation and denial but just this very process of analysis and synthesis? There is some truth in this objection. As will be shown more fully in the sequel, the act of predication, in its twofold aspect of affirmation and denial, really is a process by which we are not only enabled to add to our information about objects, but is also the process by the continued performance of which these same objects had originally been acquired, or rather produced. This needs further exposition, and will receive it in due place, as it involves a difficulty which presents itself under varying aspects at several different points in the study of Inductive Logic. At present it is only necessary to insist that extensive results of such a process must be presupposed at every

assigned time and place at which the thinker may be supposed to appeal to his Logic, unless he proposes to set to work to discuss the rational development of the human race from its first commencement: in other words to make his Logic a chapter in evolutionary Psychology. We can no more evade this necessity than we can conceive our reaching a last possible subdivision of space. Whatever example of a proposition we select contains a subject and a predicate, and one, if not both of these, will consist of an object of some kind. This remains true however far back we may insist on pushing our analysis.

To sum up, then; before the logician can set to work he must have his materials before him, and *his* materials, unlike those of the psychologist, must always be terms, or the notions corresponding to these terms. These presuppose a considerable amount of that analysis and synthesis which has been indicated above. The psychologist may afford to start with simple impressions, but the logician's starting-point must always be a stage further on. It must be the stage in which we stand in possession of 'objects', distinctly recognized as such.

II. A world of objects having thus been, if one may use the expression, roughly put together with sufficient stability and distinctness of detail for the logician to commence to exercise his art upon it, and to investigate to the utmost its unity, homogeneity and inferribility; we have next to pass in review some of the general characteristics which we must postulate in addition if that world of objects is to answer the demands we are entitled to make.

The principal claim of this description which we have to urge is best indicated by the demand that the world must be supposed to be pervaded throughout by one and the same uniform characteristic of objective certainty, existing without any limit in all directions of space and time. That is, *its* character is not to be supposed as affected in any way by *our* attitude of knowledge or ignorance towards it. The necessity of insisting upon this demand has been fully admitted by many writers in one department of the general science of Inductive Logic,—viz. in Probability,—for a rather notorious fallacy, which is sometimes described as that of 'confounding between probability before and after the event', arises almost entirely from confusion on this head. But the characteristic must be equally insisted

upon wherever we are dealing with the facts of the material world. We must recognize absolutely no intrinsic difference between the future and the past, between the near and the remote. There may be greater practical difficulties in the way of ascertaining one or the other, but in themselves the logician must regard them as being no more fundamentally distinguished than are the rails which lie before us and behind us on a railway journey. *Our* position on the track at the moment, or the direction in which we are moving, does not alter *their* character. The future and the past must be regarded as lying stretched out before our view, certain in themselves,—to use a common expression the significance of which will be better understood after we have discussed the nature of Laws of Causation,—whether we may have succeeded in determining them or not. The reader who has grown up under the influence of Physical Science will probably grant this so readily (within certain limits) that his only surprise will be that it should be considered necessary formally to state it. The sequel however will show the desirability of an explicit statement. In particular, the doctrine of Hypothetical propositions, and indeed the nature of the whole process of making suppositions or employing the particle 'if', seem to turn in part upon the non-contingent character of the universe in itself.

The question of the infinity, or rather indefiniteness of range, of the world of phenomena, though connected with the considerations just mentioned, stands on a slightly different footing. It involves certain physical generalizations, and stands in need of debate rather than of mere assumption. It shall therefore be reserved for discussion at a later stage. But so much as this can be said at the outset, that, so far as we regard the world as available for logical investigation we can listen to no speculative difficulties which would seek to put a limit upon its range of existence or possession of general uniformity. Laws of Causation,—or, more strictly, Uniformities in their widest signification,—are our only guiding clue; and if they came to an end anywhere we could not take a single step in advance. The conception of absolute beginnings or endings, of acts of creation or of annihilation, is entirely debarred to a secondary or derivative science like ours. The comparatively abstract science of Inference in general stands in

this respect on the same sort of footing with each of the concrete sciences whose most general principles it includes. Every one knows the position in which the astronomer and the geologist stand. Haunted as they are with frequent suggestions of absolute beginnings and endings, they know that they cannot explain them or even reason about them. Up to the furthest point at which scientific explanation is possible, they are bound to assume that there is no breach of continuity, but that the next step beyond is connected with the one of the moment by the same sort of links as those by which the latter is connected with the steps which went before. We contemplate the world of phenomena as if it resembled some vast scroll, unrolled to a certain extent before our eyes, but written upon in the same sort of characters from beginning to end; or rather, since we do not recognize either beginning or end, inscribed with writing which may be traced from the midst indefinitely in both directions. Of the unopened parts we guess at the contents from what we have read of the rest, though even of this opened part we can at present decipher only a fragment. But we feel that it is all, so to say, objectively knowable: that the data for knowledge are there before us: and that absolutely no limit is set to the extent over which the same sort of writing may be traced, and therefore deciphered, at some future time. In the words of Leibnitz, who seems to me to have insisted upon this doctrine most strongly, and to have appealed to it most consistently, considering the fragmentary nature of so many of the discussions which he has left behind him, "Le présent est gros de l'avenir: le futur se pourrait lire dans le passé: l'éloigné est exprimé par le prochain."

III. The next postulate which we have to make differs in one important respect from the two preceding. The former were at least true, or at any rate could not at the time be shown to be false. But the one upon which we must now insist is certainly false. We call attention to this fact thus plainly at the outset, because it is well to be frank, and because the assumption involved is intimately connected with the essential character of Material Logic as an applied or hypothetical science; i.e. as one in which we employ general principles which can only be utilized in so far as we assume a state of things which in strictness does not exist. We proceed to explain this postulate and to point out the necessity for it.

We have then already taken it for granted that the external world is largely made up of objects which exist only,—that is, exist as unities or nameable things,—as they are aggregated together and retained in the mind. So far our position is sound enough. But the postulate now insisted on is that these objects shall be the same for all intelligences, viz. for all human intelligences, with which alone we are concerned. By this it is not meant that we must assume that the ultimate and immediate sensible impressions which I and other persons experience under a similar stimulus, must exactly agree :—this is a matter for Psychology to take account of, and to answer, if it admit of a rational answer ;—but that the various groups into which I combine the phenomena, in framing objects in the mode already indicated, must correspond with the similar groups of other observers and reasoners.

The full grounds for the necessity of this assumption will only become apparent at a later stage, when we come to discuss the nature of Definition and of the Connotation of names, but its general importance can be recognized at once. We can scarcely observe or reason even as individuals, and we certainly cannot convey our observations or reasonings to others, without language. But unless language convey the same meaning to all within its range of application it ceases so far to be a proper medium of communication. And for the purpose of such communication it is essential that we should all have the same set of objects before us to observe and name. Not only is it possible for language to mislead, by our misapplying names to the objects which we have clearly before us, but it may also fail by our simply not having really the same objects before us. Take the following illustration. Any one who looks upon a surface of stormy sea will not fail to see it divided into a number of tolerably distinct 'objects', i.e. waves. And any other observer standing at his side will see it similarly divided, that is, he will perceive the same set of objects. But this agreement of observation depends in great part upon contiguity of position. If one person were on shore, and the other at the end of a long pier, they would *not* see the same objects. A few of the larger waves might be identified by both, but the observers would differ as to the limits of these, and as regards the rest they would differ widely. That is, one and the same

mass of materials would be grouped inte more or less completely distinct sets of objects. And if we were to conceive the observers trying to communicate their observations by language, the very foundation of all language, viz. common reference of sign to thing signified, would to some extent fail them. A clearer illustration of this variety of simultaneous perception by different observers might be drawn from the clouds. Two observers standing on the same spot would closely agree as to the number shape and relative magnitude of the fleecy clouds in the summer sky. But if they were communicating by telephone, at a few miles distance, each would probably find it impossible even to identify any one of the individual objects which the other attempted to describe to him. These, of course, are extreme instances, but the requirement which thus fails here is yet necessary as a general logical postulate. We cannot either convey or receive information by propositions; we cannot express our uncertainty by help of a question; unless the words which we employ stand for the same things.

How far, then, is this requirement secured as a matter of fact? We must look first to the present, and then to the past and the future.

(1) So far as regards the present, there can be little doubt that the vast majority of people do see the world very much in the same way: at least as regards the principal objects which compose it, and about which we have most frequently to communicate. The same individual objects are recognized by us all; we class them into the same general groups; and we analyze them into the same component attributes. But we must not suppose that this harmony is brought about by any intrinsic necessity, beyond that which arises out of our common experience. Certain simple natural objects, such as the sun and the moon, and most living beings, will take care of themselves; and it is easily seen, in the case of infants, how soon the same sort of individualization takes place, especially when we are dealing with any object which can readily be moved about amongst its surroundings. But when we proceed from such instances as these, where the element of mental construction is relatively small in comparison with that which is forced upon us by nature, to instances in which this element is relatively very large, we find that some other aid than individual sense

and judgment has to be invoked. Such an aid is found in language. The main reason why we recognize the same world of objects before us consists largely in the fact that we are social beings in possession of a common instrument of communication. Language has a powerful influence in steadying or averaging our perceptive faculties. It acts upon us both individually and collectively. In regard to each individual it aids, as Locke long ago pointed out, by holding together the constituent elements of the more complex objects, and thus enabling us to see again what we had seen before. And in regard to any particular society as a whole, it plays a large part in compelling each of us to see the world as his fellows see it; for it gives the pressure required to convert a near approach to common perception into an almost complete identity. Add to this that each of us, being born to the use of a language, learns to name many objects simultaneously with first seeing them, and thus inherits the general arrangement of objects which had been gradually worked out by his predecessors. In this respect, to use a mathematical illustration, Language prevents sudden discontinuities by securing that at every moment the initial direction of variation shall be the same for all, however much their private experience may commence, subsequently to that moment, to vary it. Had each one of us been forced to commence anew for himself that process of analysis and synthesis by which an objective world is built up, we should probably differ amongst ourselves quite as much as the occupants of Bedlam,—one of whose principal characteristics is their greater spontaneity and independence in the process of object construction—now do from us and from each other. Under the actual circumstances of life, we think and speak under the powerful constraining influence of a common speech, and hence we see and think so nearly alike; though it must always be remembered,—what comes out clearly enough when we begin to define our terms,—that as wide a deviation of perception may actually exist under common terms, as of doctrine under common creeds.

(2) But when we proceed to look a long way back in time or a long way ahead, a very different state of things has to be contemplated. The thorough-going objective or material view of the world,—that, for instance, of the ordinary man of science, —is somewhat of the following kind. We fully admit, or rather

assert, that enormous changes have take place during secular periods: the introduction of new species and extinction of old, the geological and climatic changes which the ages have worked out, the rise and progress of social life with its infinitely varying and extending complications, and so forth. But, when we contemplate these changes, it is throughout *our* conceptions, —those of the student and lecturer of the present day,—which we are applying to construct and to explain the past course of events. To us the universe, when there was no rational being in it, is the picture of the action of physical forces as *we* should observe them if we could be put back into that period. The customs of the savage and the conceptions he entertains, are what *we* should make of them, and so forth. To the scientific man this is quite justifiable, since his purpose avowedly is that of explaining the past from the point of view of the present, and since he does not trouble himself more than is absolutely necessary about the language and conceptions in which things might be variously described at different times. The logician's standpoint, however, is somewhat different. He has made it his main business to consider objects through their names, or through the conceptions corresponding to these. His doctrines of the Categories, of Connotation and the Definition of names, all imply this attitude;—in fact an old description of Logic was that it 'referred first intentions to second', that is, that it was the function of Logic to arrange and infer the facts of nature in accordance with current conceptions or notions;—and therefore the contrast between the way in which one generation or another views the world, is necessarily brought under his notice.

As already remarked, there is no trouble and dispute about the simpler and more obvious objects; they must always have been very much the same as they now are, at least to the mere observer who does not seek to analyze or account for them. But in the case of those more complex objects in which mental construction plays so large a part, it is by no means mere quibbling to raise the objection,—'But there *were* no such things then: inasmuch as objects consist in great part as they are perceived, conceived, and named, they simply do not exist to those who do not perceive them and have therefore never thought of naming them.' To this objection the same answer may perhaps

be suggested as is so often given to an analogous difficulty in the Berkeleyan hypothesis of immateriality. It is sometimes urged, for instance, as indeed it was from the first,—'Then, the essence of objects lying in our perception of them, it follows that before there were any perceptive powers there could have been no objects: i.e. there could have been no material world before man existed upon it.' The usual answer to this objection is thrown into the hypothetical form,—'We mean that *if* there had been any being with perceptive organs like ours he would have perceived the world just as we do now[1].' I am far from thinking this answer satisfactory; in fact it seems entirely to evade the difficulty. The objection starts from the postulate that there *were* no human beings at the time in question, and infers that consequently there was no material world. The answer really starts from the postulate that there *might have been* such beings, and replies that if such there were the world would have been perceptible to them.

In the case before us, however, such an answer is fair enough; or rather it is not called for, since the difficulty it is intended to meet is frankly admitted. The very point we are here urging is that many of the objects which fill our categories, and answer to our general names, simply did not exist in the days of our earliest savage ancestors. But this does not signify; because the position we take up is that in these matters, so far as Logic is concerned, the present is to legislate for the past. We fully admit that such and such objects were not perceived, so that the corresponding notions and names did not exist; yet still we consider them to have been existing because we know that they would or might have done so to us. Had we been there we should have seen them. That is, what we do is to project our own present view of the world into remote times and places. We thus postulate a world, or aggregate of objects,—not out of relation to human faculties in general, which would of course be absurd,—but conditioned in relation to one representative state of faculties, namely that of our own time and society. We conceive some mature mind, at the present standard of civilization, and we assume that such a constitution and arrangement

[1] This is the answer given by Mill (*Exam. of Sir W. Hamilton*). Berkeley's Theism, of course, gave him a second string to his bow, and a much stronger one: the world, he maintained, had always existed in the mind of the Deity.

of the detailed phenomena,—such objects in fact,—as would present themselves to him, are to be regarded as universally recognizable at all times. And this is quite fair, for our logical scheme is avowedly constructed from the present point of view. It does not, or should not, profess to be anything else than an interpretation of remote times by the schedules and forms of our own time.

So far then as regards the past, however remote the period to which we recede, there does not seem to be any serious difficulty; but it must be remembered that there is also a future to reckon with, and the application of the same principle in this direction will bring about results that deserve some scrutiny. The past, as remarked, offers no difficulty. We are well accustomed to observing and reasoning for others in a way which we know to be far over their heads, and therefore we can readily extend our categories and terms backwards into times and places where they could not originally have gained acceptance or even appreciation. To imagine how men of the past might have entertained our ideas and used our terms, though in fact we are aware that they did nothing of the kind, is a process of mental *expansion* or progress, and is so far in order. But clearly if we are to take up the same attitude towards those who come after us, we should have to adopt a process of mental *curtailment* and retrogression. It would be absurd to suppose that our way of regarding the world can be final. Future generations will completely set aside our classifications, and will find very many of our notions and terms quite unsuitable to express their way of regarding and grouping the facts. At some future stage they will presumably stand to us, in respect of mental development, much as we do to the prehistoric savage.

Now as we clearly cannot raise our present notions up to the standard of the future, we must adopt the opposite course and estimate the future by the narrower standard of the present. Hence, when any one lets his imagination wander into the abyss of the past and the future, he must remember that he is really behaving in a somewhat different way in these two directions. He may justify his modern point of view, in the former case, on the ground that our ancestors at any rate might, though they unquestionably did not, see what we with our eyes should

see. But to suppose our remote successors to see things as we see them is to suppose them to consent to a deliberate retrogression[1].

A single example will serve to explain what is meant by this, and as it is offered merely for illustration it may pass as such even with those who reject its correctness. Take the case of *Religion*, with all the group of notions involved in this general term. We can trace the evolution of sentiments which we should now refer to this head back to the remotest limits of savage life. But it needs very little consideration to convince us that the terms in which we express our statements on this subject, though they indicate tolerably definite notions to us, would not do so to very primitive men. It is *our* grouping, not theirs, of the sentiments which form the raw material of all objects of thought. Could those men have risen to our standard of intelligence whilst retaining their own standard of belief, they would probably have utterly refused not merely to accept or deny our statements but even to realize the admissibility of the very terms in which they are couched.

The difficulty above discussed will perhaps be better appreciated if we revert for a moment to a phraseology which I have rather avoided in this work, and which seems to me to be likely to be superseded in the present predominantly objective and material treatment of Logic. Some logicians still hold, and nearly all used to hold, that what they have to deal with in their science is a stock of ideas or notions; the act of judgment consisting in the combination of two such elements. The sum total of all such 'concepts', to use a current modern term,

[1] Some years ago a discussion was started in the newspapers as to possible communication with the supposed inhabitants of Mars. It was taken for granted that their mental condition would be on the same general level as ours; or at least sufficiently nearly so to render inter-communication feasible. Is there rational ground for such a belief? There are astronomical reasons for supposing that Mars started on its career as a planet long before the earth, and that it may in consequence be millions of years in advance of us. On the general principles of Evolution, the inhabitants of Mars, if any such there be, may be mentally separated from us by a gulf far more profound than that which lies between ourselves and the savage of the Palaeolithic age. They may stand to us somewhat as we do to the brute creation, and have long since learned,—not from us, but by means of us,—whatever may serve their purpose as to our attainments. What would be thought of a proposal, on the part of the monkeys, to start "inter-communication" with the naturalists?

or '*voces*' to use a common old term, forms so to say the stock in trade of every logician. All logical processes,—judgment, reasoning, definition, and so forth,—are nothing but the combination, justification, and analysis of these notions. Now it is perfectly undeniable that such an aggregate of notions as this is strictly conditioned as to time and place,—for notions can only exist in so far as they are entertained in the mind,—and therefore few if any of those now in currency can be really identified with such as were entertained by our primitive ancestors. Speaking objectively, as we did before, the reader might have some trouble in conceiving how 'things' could be said not to exist, because there was no one then to perceive them. But when we use the really equivalent expression of 'viewing objects under such and such concepts', we see more easily how necessarily all our statements are couched in the frames or forms of the present day.

IV. Another difficulty, of a very distinct kind from that last noticed, but which is an equally serious one from a speculative point of view, must now be discussed. The reader will have gathered that what the logician strives after is the attitude of the observer or judge, pure and simple, who contemplates the world for the purpose of drawing inferences about it. He is to stand entirely apart; his function being to think but not to act; to observe but not to influence. We have seen, just above, that we have to regard his present standing-point as a sort of representative one which is to serve equally well for any other time or place; though it came out in the course of enquiry that the attempt to secure such a representative or common standpoint involved a certain anachronism both prospective and retrospective. There did not however, so far, appear to be any inevitable inconsistency in the mere attempt to take up this purely contemplative position. There did not seem any reason against his aiming at his ultimate ideal of framing a complete mental reproduction of the entire course of events from past to future.

Strictly speaking however there is such an inconsistency necessarily involved, which presents itself in the following way. Look at that complex of phenomena which constitutes the logician's world, with all its aggregate of objects which furnish his examples and exemplify his proofs. These are certainly not

supposed to be confined to material objects, but must equally include the thoughts, feelings, and actions of human beings; for every event without exception which we can suppose ourselves observing may become a logical element. It may stand as a subject or predicate, and it may give ground to an inference. This catholicity of application is in strictness true of every system of Logic, for even on the narrowest formal view of the science we may draw our examples from the conduct and character of our fellow men. But in a system of Inductive Logic, especially when this embraces the so-called Sociology, we are much more largely concerned with the doings of men, and the inferences we can draw as to their conduct. Now as soon as we proceed to do this we find ourselves confronted by a troublesome difficulty. The agents whose performances are thus supposed to be a part of the object world of our logician: are they themselves allowed to be logicians? and if so how can they simultaneously occupy the position of observer and that of being the subject of observation? Any strict view of the logician's stand-point,—when, as now, we are defining it with the utmost accuracy,—is certainly inconsistent with such a supposition. He is assumed to take up a contemplative, not an active, position. He has to stand aloof from the phenomena in order to observe, judge, and infer. He must not simultaneously try to form a part of his own observations and inferences; for if he does he will almost certainly introduce a disturbance into them which will invalidate the inference.

It must be admitted that so far as the direct and actual performances of the observer are concerned, the inconsistency here indicated produces no serious results. The department of speculation in which it does give rise to real difficulty is that in which *Hypothesis*, in its widest signification, has to be resorted to. And it is mainly with this reference in view that we now insist upon the difficulty. In a future chapter, which will be devoted to the discussion of Hypothesis in general, it will be pointed out that,—not merely in Ethics, but in what may be called the Science of Human Conduct in general,—this unauthorized transference of himself, by the observer into the midst of his observations, is very difficult to exclude, and causes serious logical inconsistency. Within the domain of Physical Science, and over the range of the larger part of human action

of the ordinary kind, the difficulty in question is admittedly but slight, and it claims attention here rather from the desirability of complete scientific accuracy of definition than for the purpose of avoiding any actual mischief or error.

At the same time, as we are upon the subject, it is just worth pointing out that the complete attainment of the ideal position of the mere observer is nowhere to be secured even in Physics. Take, for example, the most extreme case, where this position may seem to be completely securable, viz. the science of Astronomy. Here, if anywhere, the observer might conceive himself standing entirely apart from the objects whose motion he calculates; picturing mentally their career without interfering physically with it. He would claim, apparently with good reason, that he merely watches what they do, and that as he cannot possibly experiment, he cannot in the slightest degree interfere, with their motions. No more he does, so far as any results are concerned which the utmost attainable refinement of observation, or indeed any refinement vastly beyond what is attainable, could ever detect. But this does not hold if we like to take account of influences which are undeniably real, though so inconceivably minute that it would be absurd to notice them except by way of illustrating a point of theoretic interest. It cannot be denied then that if the Law of Universal Gravitation is rigidly true the calculator *does* influence the course of the planets themselves, and does so by the fact of observing them. Every motion to or from his instrument, nay the very calculations he writes down on paper or the words he utters by his voice, involve motions of matter, and therefore react on the motions of every other material thing in the universe, including the planets themselves. Accordingly in calculating their motions as a passive spectator he is in perfect strictness disturbing those very paths which he had calculated, and consequently falsifying his own conclusions. This impossibility of complete isolation of observation, existing thus as a speculative truth even in the case of those objects which are physically the most remote from us, assumes more and more of a practical aspect as we proceed in the direction of volitional human actions, and it reaches (as we shall see hereafter) a climax when these latter are treated not as actual but as hypothetical.

There seems to me to be only one way of meeting this

difficulty so as to make our position speculatively free from inconsistency. We must start with a fiction which may as well be definitely stated as one of the postulates of Logic. No living human being can be spared to occupy that purely speculative position which is wanted for our logician. Each one of us has his own position amongst the objects which compose the world; he has his own little sphere of activity which he may quit only by taking up some other. No one of us can be spared to occupy the ideal logician's seat; and if he tried to do so he would find that he was perpetually leaving it, and mixing himself up in some way or other in the course of what should have been to him a wholly external world. What therefore we have to do seems to be this. We have to assume a sort of representative mind, distinct from any one of ours, but endowed with the same conceptions (and of course laws of inference) as we at present possess. For such a mind as this the ideal position of absolute non-interference with the objects before it, which is denied to any of us, could be rigidly preserved. And when the logician claims, as he sometimes explicitly does, that he has no other function than to observe and judge and infer, he must in consistency create for himself such a fictitious post as this.

It may seem to some readers little else than a waste of time to have enunciated and discussed two such postulates as those which have just been laid down. This course has however been adopted deliberately; partly in order to secure perfect speculative consistency, and partly also to prepare the reader for the extreme importance of that general view of Logic in pursuance of which it becomes necessary to remove even such apparently far-fetched difficulties as these out of our path. We proceed to add some pages of illustration of this general view under a new head.

V. What we have to take for granted in Logic is, then, a duality: an external element and an internal. On the one hand, outside us, there is the world of phenomena pursuing its undeviating course; and, on the other hand, within us, there is the observing and thinking mind. Logic is concerned with the judgments of the latter about the former. The entire omission of either of these two elements,—if indeed this were possible,—would involve the destruction of the science, as, on the other hand, any undue stress upon either leads to confusion and to

inconsistency. The thorough-going retention of this duality is one of the leading characteristics of the whole treatment adopted in this work. Its extreme importance will only gradually be appreciated as one doctrine after another comes up for discussion, and as we find ourselves influenced in our decision about each by the principle in question; but a slight sketch indicative of its significance may conveniently be given at once.

Logic then as here conceived is neither a purely objective nor a purely subjective science. It involves both elements, consisting essentially in the relation of one to the other, and serious error results from the neglect of either aspect, and even from insufficient recognition of it.

(1) Consider, for instance, what would follow if we were to propose to drop the mental or subjective side. Such a proposal has been made, and has even been incorporated into the definition of the science. Thus Mr H. Spencer lays it down that "Logic formulates the most general laws of correlation among existences considered as objective", that "Logic, instead of being a science of certain subjective correlations, is a science of certain objective correlations[1]." Strictly maintained, such a view as this would confine us to a bare statement of those objective laws or regularities which lie at the base of all inductive inference. It would deal with exactly the same subject-matter as that with which each of the special physical sciences is concerned, though it would be more comprehensive in its range than any one of these, covering in fact the ground common to them all. Just as each special science treats the laws distinctive of that group of objects which assigns its unity to the science in question, so, it may be urged, can we treat of the uniformities common to the sciences in general, and regard them as forming a single science, viz. Logic. In both cases alike our faculties of observation and reasoning have to be taken into account, but they are only recognized tacitly or indirectly. Their existence is not expressly noticed except when it is considered that they are likely to become sources of error and confusion.

[1] *Principles of Psychology*, Vol. II. pp. 87, 100. The difference between us perhaps is not quite so sharp as these passages would indicate, since Mr Spencer proceeds (as I understand him) to make a distinction between 'Logic' as objective and the 'Science of Reasoning' as subjective.

It appears to me that such a view as this is insufficient, and would, if consistently adhered to, lead to the rejection of much of what has always been regarded as forming a part of the subject-matter of Logic. It seems indeed obvious that any attempt to confine ourselves to a bare statement or analysis of facts of nature must be insufficient when what we are concerned with is *inference* about those facts; for inference turns mainly upon the distinction between what is known and what is unknown, and this distinction does not lie in the facts but in our appreciation of them. I quite admit that all science involves this element, but it does so indirectly; it does not make this element its express subject-matter. For instance any treatise on Astronomy must involve certain relations to the current standard of attainment and information at the time. It will not state what is already perfectly familiar to every one, and it cannot state what is unknown to any, so it deals largely with what has been comparatively recently acquired. To this extent the purely objective treatment is conditioned by subjective considerations; but such a reference is subordinate and indirect.

Now when we turn to Logic we find that our treatment is conditioned by such considerations in a very different sense; for the current state of attainment as regards knowledge is not here an inevitable accident but is necessarily involved in the treatment of our subject-matter. Look, for instance, at the distinction between the 'essential' and the 'accidental' attributes of anything, upon which the whole significance of Connotation depends, including amongst the consequences the doctrine of Definition, which has always formed one of the central parts of every system of Logic. If we objectify too much we simply annihilate this distinction. Beyond a doubt the essential and the accidental attributes are both, as the phrase runs, 'in the facts', but the distinction between them must be sought, not there, but in our estimate or appreciation of those facts. Attempts have indeed been made,—which will receive notice in their due place,—to evade the necessity of any such appeal, but as it seems to me without avail. No distinction will really satisfy our requirements which does not involve the admission that the essential attributes are those which are 'universally recognized', or at least so far agreed

upon by all reasonable authorities as to be 'implied in the use of the name', or which does not involve in some equivalent way a conventional standard of attainment in respect of the significance of the name. And this holds good throughout our treatment of the whole doctrine of Definition under its various aspects, such for instance as the distinction between Real and Verbal propositions. We are perpetually encountered, in all these discussions, by the necessity of admitting a distinctly subjective element in the way of a conventional or normal selection of the facts as distinguished from the mere occurrence of the facts themselves.

Where, however, these considerations become most prominent is in the treatment of the syllogistic process. For instance the statement is frequently made, and has found its way into works of merit, that no new truth is ever reached by reasoning; or, in more cautious and restricted language, that every syllogism is a *petitio principii*. In any intelligible sense of the words the former statement seems palpably absurd. De Morgan meets it in his usual happy style by the reply that "persons not spoiled by sophistry will smile when they are told that knowing two straight lines cannot enclose a space, the whole is greater than its part, &c.,—they as good as knew that the three intersections of opposite sides of a hexagon inscribed in a circle must be in the same straight line. Many of my readers will learn this now for the first time: it will comfort them much to be assured, on many high authorities, that they virtually knew it ever since their childhood" (*Formal Logic*, p. 45). This is conclusive as against those who do not hold that geometrical reasoning is largely a process of intuition; but, if this objection against its applicability be raised, we have only to take a few of the complicated propositions which the Symbolic Logic will readily furnish. Draw up a small group of these, and side by side with this set down some remote conclusion deducible from them, and ask the ingenuous reader if there is nothing 'new' to him in the latter however clearly he understands the former. The conclusion may follow as a consequence from a few propositions which in themselves are admitted readily enough, but if we are to allow the objection in question we must either maintain that the conclusion is not new or maintain that it was not reached by reasoning.

What is intended by those who use such an objection as that in question is probably this. They mean that the conclusion is, so to say, *in the facts*, equally with the premises; being indeed nothing else than those very premises, or a portion of them, differently worded. Mill himself uses this argument in a narrower application, when contending that simple conversion of a proposition is not inference, because there is no new fact involved. In other words, given better powers of comprehension or intuition, we might directly perceive the conclusion in the premises, just as we perceive the import of the premises separately. This is certainly true; but then, in this sense, *all* knowledge is lying there before us in the facts. The riddle of the world in general, along with all minor puzzles, is there sure enough, only unfortunately we cannot make the virtual knowledge serve the purpose of knowledge which is real.

The acceptance which this opinion has received is probably largely due to the almost absurdly trite and obvious examples by which the syllogistic process is commonly illustrated. This will occupy our attention hereafter when we come to discuss the Syllogism. All that we are now concerned to establish is that the distinction between what is known and what is not known is essential to Logic, and peculiarly characteristic of it in a degree not to be found in any other science. Inference is the process of passing from one to the other; from facts which we have accepted as premises, to those which we have not yet accepted, but are in the act of doing so by the very process in question. No scrutiny of the facts themselves, regarded as objective, can ever detect these characteristics of their greater or less familiarity to our minds: we must in addition introduce also the subjective element if we wish to give any adequate explanation of them.

(2) So much then, for the present, as to the results of attempting to over-objectify the science. On the other hand, when we underestimate the objective element the consequences are quite as mischievous. It has, of course, never escaped the notice of logicians that some original appeal to the world of phenomena was necessary in order to acquire our data, and to secure any value for our definitions. All logicians have admitted as much as this. But a much closer and more

continuous appeal to the external world is demanded in order to carry out our system to any satisfactory results. The whole Theory of Induction, for instance, and the processes of Rational Classification, demand continual resort to nature at first hand. No mere introspection, and no rules which do not go beyond simple consistency nor attempt to grapple with the true and the false, can avail us here. As to this there is but little need to insist, for nothing can be more complete than the frank avowals, in fact the claims, of such a consistent writer as Mansel. A considerable portion of his *Prolegomena Logica* is occupied with an almost contemptuous refusal to admit one application of the science after another which have commonly found acceptance amongst logicians; and, in regard to what he does admit, he is certainly free from any charge that he has thrown light on the processes of Induction or of Classification.

We need hardly remind the reader that the remarks in the last few pages are not offered as an adequate discussion of the points involved, but are meant to prepare the way for future reference. They are intended to indicate how many and important are the consequences of the general position here maintained, viz. that a comprehensive system of Logic must postulate, must in fact take as its basis, a fundamental duality. This twofold aspect of the science,—objective and subjective,—is so important a characteristic that it will be perpetually presenting itself in various applications throughout the course of this work. It seems to me almost peculiar to Logic amongst the sciences. There are some sciences, like Psychology, in which the primary reference is throughout to the mental processes; and there are others, like the ordinary physical sciences, in which the primary reference is throughout to the external phenomena. But a science like Logic, which has to do with the processes of the human mind when judging about phenomena, and, more particularly, with the process of gradually extending our knowledge of those phenomena, occupies necessarily an intermediate position. The treatment here adopted may indeed by comparison be called Material or Objective,—I have chosen to insist here and elsewhere upon the convenience of this designation of Logic as here conceived,—but it must be remembered that the epithet is mainly employed in order to mark the departure from the extreme subjectiveness

of the customary treatment. If it were not for this bias of traditional treatment against which we have to press, it would be inappropriate to adopt a designation which implies closer affinity with one side of the duality than with the other, for the neglect of either distorts and damages our view of the whole.

VI. The next postulate we have to discuss follows as a direct consequence of the acceptance of the above duality. It is best described summarily by the double statement that we must not only recognize the distinction between the true and the false, but that we must also have decided in any given case what sort of test we intend to adopt in order to distinguish between them. It is desirable expressly to call attention to this twofold way of stating our requirement, because there really are two very distinct questions involved whenever we speak of logical truth and falsehood. I cannot but think that it is greatly owing to a lack of appreciation of this distinction that we find such extraordinary diversity of opinion amongst logicians as to whether or not they have any business to take truth and falsehood into account. Whereas some writers (for instance, Mill) declare that "it is only as a means to material truth that the formal, or to speak more clearly, the conditional validity of an operation of thought is of any value", and that the consideration of the former is "Logic κατ' ἐξοχήν, and anything else called by the name is only ancillary to it" (*Exam. of Sir W. Hamilton*, pp. 402, 3); we have on the other hand Jevons curtly remarking in reference to an examination question which had touched on this point (by enquiring 'whether predication involves real existence?') that it "must have been asked under some misapprehension. The inferences of formal logic have nothing whatever to do with real existence; that is, occurrence under the conditions of time and space" (*Studies in Deductive Logic*, p. 55).

(1) To clear our way through this confusion it is essential to divide the enquiry into two parts. The first of these concerns the general necessity of admitting the distinction between the true and the false; between what does exist and what does not. This necessity springs directly from the postulate last considered. Start with a world of phenomena, on the one side, and an observant mind contemplating this, on

the other, and there arises at once the possibility of agreement or diversity between the two. In the mere phenomena there is nothing which can be termed true or false. Equally so in the mere notions which we entertain of the phenomena. To produce the distinction in question these two elements have to be brought somehow into relation.

In saying this we need hardly remind the reader that we have already, in our first assumption, decided to pass over the fundamental question as to the ultimate criterion of truth. In Logic we take the world substantially as it appears to us, that is, as it is given to us in sense. We leave to Metaphysics the question of the ultimate validity of sense and consciousness: what exactly they tell us, and with what certainty. We have to stop short of this primary stage, and we understand by 'truth' the agreement of our notions with the testimony of sense. This agreement must be briefly considered in its application, first to terms, and secondly to propositions.

(i) First then as regards the term, or rather,—what will be more convenient to treat here,—its corresponding mental element, the notion or concept. Every notion we entertain must either be in harmony with its supposed object, or not. The work of verification may of course be a tedious and delicate one: it may even be one which in our present circumstances we are unable completely to carry out. But we must always presuppose that the process of verification is conceivable, whether or not it be, in any particular case, feasible. A full account of the process must be sought in Psychology, so that nothing need be added here beyond the few remarks necessary in order to obviate misunderstanding. Remember then that we are in no wise concerned with the question which for ages perplexed philosophers, viz. in what sense our ideas 'resemble' or are 'copies of' actual external objects. All that we compare is the impression at first hand and at second hand, the presentation and the representation. When I recall an impression I have formerly received through sense, the recollection should resemble (so far as such resemblance can extend) the original sensation or group of sensations. When it is one which has not been experienced before, it should resemble what would be sensibly perceived under the right circumstances[1].

[1] I need hardly guard myself against being supposed, in saying this, to

As regards the verbal expressions by which we should indicate the distinction in question there is some diversity of usage. Where *propositions* are concerned, for reasons to be presently pointed out, the antithesis between true and false is universally recognized; but there is no such agreement where terms or notions are in question. 'Real' and 'Imaginary' are the correlatives most often adopted, and they will therefore be employed here, but it must be admitted that they are not very appropriate. For one thing, as is often pointed out, every notion, whatever its character, is equally 'real' in the sense of being actually present to the mind at the time. So again every notion, whether correspondent to fact or not, is imaginary in the sense of being entertained in the imagination at the time. What of course is wanted is some pair of correlatives which shall do nothing more than intimate that some of these notions thus actually entertained in the imagination do, and others do not, conform to fact:—whatever the nature of that fact may be, and whatever the test we may adopt to discriminate it. In default of any thoroughly appropriate designation we shall not refuse to adopt any of those currently adopted, such as,—true and false, valid and invalid, real and fanciful or phantastic, correct and incorrect, real and imaginary,—with a preference for the last as most familiar.

(ii) Turn now to the proposition. The connection between this and the elements into which it is grammatically and logically resolvable, viz. the terms or notions, is very close. In fact we might almost go the length of asserting that the distinction between proposition and term is only clearly marked when we are communicating with others, or are very deliberately reflecting within ourselves. Moreover in these cases the distinction is not universally existent: it is, for instance, lacking in some of the ruder languages, at any rate in the form in which we are familiar with it. In any case the connection in question is a close one; for the notion may often be regarded as having been acquired by a succession of judgments which gradually built it up, whilst the proposition is always ready to resolve itself into the two notions which constitute its subject and

entertain the doctrine of Hume that all ideas are *nothing but* copies of original sense impressions.

predicate. Just then as every notion must be real or imaginary, so must every proposition be true or false.

There is one point however here, arising out of the greater comparative complexity of the proposition, which requires notice. The notion is a single element, and therefore must either agree with its object or not. But the proposition is one degree more complex, and we must accordingly introduce a refinement. The true proposition should, in perfect strictness, be regarded as one in which both elements, subject and predicate, can be justified as notions, in addition to the justification of their union,—or non-union, if the proposition is a negative one. But this leaves some alternatives over, which are generally dealt with by assuming that in considering the truth of the proposition we may take it for granted that the simpler elements,—viz. the separate notions,—are already guaranteed. The false proposition would then be one in which these separate elements are accepted, but in which their union is not accepted. If, on the other hand, the separate elements also were rejected the proposition would not so much be regarded as false, but rather as imaginary because its materials were so.

The reality of a notion corresponding in a general way to the truth of a proposition, it may be enquired why (as already indicated) we are so much more in the habit of employing a single recognized expression to mark the antithesis, between what is right and what is wrong, in the latter case than in the former. The reason, I apprehend, is mainly this:—that the proposition is the medium for the communication of our ideas to others. Much of our private thinking is done, when we are awake as well as when we are dreaming, by a mere succession of notions and images flitting through the mind; but as soon as we want to pass out of ourselves and to communicate with others we almost invariably adopt the propositional form. Now such contact with others is the usual and surest mode of testing our thoughts. If we could conceive it possible that a perfectly solitary thinker should grow up from infancy to anything approaching the ordinary cultivated standard, we may feel sure that his clearness of distinction between truth and falsehood would lag far behind his other attainments. It is mainly through interchange of thought with others that the distinction is aroused and kept awake in our minds, so that to

many persons it seems as if truth and falsehood first come into play when we deal with definite statements. And this explanation is confirmed by the different appreciation popularly entertained of those who transgress in either way. He who habitually and wilfully deals in incorrect statements, whether or not they mislead others, is apt to obtain the harsh designation of a liar; whilst he who with equal wilfulness persists in cherishing unfounded notions is seldom exposed to worse epithets than those of dreamer or builder of castles in the air.

One objection may be noticed here, in passing. It may be said that we are only admitting a twofold division where we should admit a threefold one. Between the true and the false, or the real and imaginary, we must be prepared to interpose the doubtful. Suppose we simply entertain a notion, or frame a proposition, at random: to which class is it to be referred? An adequate answer to this enquiry would involve a discussion of the Theory of Probability, whose special function it is to treat of doubtful propositions whenever they have any scientific value; that is, whenever they can be quantitatively estimated. All that need be said about them here is briefly this. When a notion is put together entirely at random, or has its time and place conditions assigned to it without any grounds, it may as thus regarded be pronounced imaginary; on the ground that infinitely numerous as are the objects in nature those which we can conjure up in fancy are infinitely more so, and that therefore the odds are enormously against any particular notion being correct. Fully recognizing that it may possibly be real, we say that it is best treated as merely fanciful. The *proposition* stands on a slightly different ground; and there are reasons in certain cases, but in certain cases only, for saying that a proposition uttered at random is as likely to be true as to be false.

(2) The above remarks will, we hope, have made it plain that the distinction between the true and the false, that is, between what is and what is not to be admitted, cannot be dispensed with in Logic. It forces itself upon our notice at every stage and in every department of the science. But this admission leaves it open to us to decide upon the test or standard of truth: it does not necessarily commit us to the exclusive acceptance of that one supreme scientific or rational

test which consists in the ultimate justification of human conduct. The failure to recognize this fact seems to have been at the bottom of much of the confusion and difference of opinion amongst logicians as to whether it should be held to be any business of theirs to attend to the truth of their premises.

There are three distinct tests or standards which have to be considered as being at our service. I am acutely conscious of the disadvantages attendant upon any mode of speaking which would lead to the belief that there could be different kinds of truth in any strict sense of the term: but there does not seem to be any other convenient form of expression which would not be open to greater objections in other directions. Whatever expression however be employed here it is hoped that the careful reader will not be led into any mistake as to the paramount importance of the only sense in which truth can be said to be the ultimate end of all investigation and reasoning. It is only because Logic is an intermediate or ancillary science that the variety of tests to be here pointed out becomes admissible.

(i) For one thing, then, we may take as our standard that of mere conceivability; that is, we may accept for logical purposes whatever the mind can conceive. This view may be supported on the ground that such a standard is the only one which every one can be supposed permanently to carry about with him, so as to be able to appeal to it without further resort to experience.

This is a purely formal or à priori test, and clearly the only one of that particular kind available for us. Such a standard is openly supported by one school of logicians,—that of which Hamilton and Mansel are the best known exponents in this country,—though it is also from time to time admitted by other writers whose general schemes are by no means in accordance with these authorities, and who do not seem clearly to appreciate the consequences of such an admission. The simplicity and generality of such a test must be allowed, but these merits are far more than counterbalanced by the large excisions from the scope of the science to which its adoption forces us. For instance, when thoroughly carried out, it simply renders all reference to the extension or denotation of our terms meaningless; for such extension becomes in every case

alike infinite, that is, potentially applicable without any limit to the infinite array of conceivable objects of the kind in question, and consequently no particular occasion for taking the extension into account can well arise. Physical notions would in fact stand on much the same footing as those of geometry. Here,—when for instance we are dealing with any possible kind of curve,—we know no distinction between what is merely imaginary, and what has its counterpart somewhere in nature. Consequently there is no appropriate opening here for logical 'extension', which always implies some actual limitation. Where everything has always the fullest extension attainable, there is no need to take any account of the extension at all.

The logically real concept then, on this view, is any concept which we can conceive; that is, any which we can in the last resort intuit or imagine. As regards the corresponding truth of the proposition,—or judgment, as writers of this way of thinking generally prefer to express themselves,—matters are also considerably simplified. Any two notions which the mind can unite together,—that is, any two of which one does not actually contradict the other,—are admitted into combination to form a judgment. I do not mean to imply that such writers are in the habit of speaking much of truth and falsehood in this connection: on the contrary a sense of consistency generally makes them prefer to substitute some other expression, and to speak for instance of 'valid' or 'admissible' judgments.

It is when we come to deal with the third logical element, viz. reasoning, that consistency is apt to give way to expediency. Carrying out the same general test of recognizing all that we can think or imagine, it would seem to follow that all reasoning should be admitted which does not actually contradict itself. That is, every proposed conclusion should be accepted if we *can* hold it together with the premises, not those only which we *must* thus hold. Thus, for instance, 'undistributed middle' would pass muster, but the substitution of a negative conclusion for the affirmative in *Barbara* would be rejected; because the former might actually be consistent with the premises but the latter cannot. But such a treatment as this has been avoided by every logician, and it has been agreed to set up the more rigid standard of admitting no reasonings except those which

must hold good, that is, which we cannot conceive being other than they are. With this last exception the test in question, namely that of mere conceivability, seems a perfectly consistent one. It is, as Mill remarks in his criticism of Hamilton (*Examination*, Ch. xx.), a merely negative test. It simply excludes notions, judgments, and reasonings, which we know without any specific experience must be wrong, but declines to draw any further distinctions within the wide area thus left open.

Now what we must insist upon is that even here, with this immense latitude of comprehension, the essential distinction between the true and the false still forces itself upon our notice. The false notion doubtless is perfectly harmless, for, since we cannot conceive it, we most certainly never can meet with anything corresponding to it. To forbid it may seem like forbidding a crime which no man can commit. Practically therefore we have removed the false out of our sphere, by the simple expedient of admitting as true all that the mind can grasp or experience can encounter. The distinction therefore seems to be obliterated, because everything we can experience or suppose is confined to one only of its two divisions. But the moment we come to the judgment we find the state of things to be otherwise. This is a point which needs notice here, but its full significance only emerges in the department of generalized Formal, or Symbolic Logic, where the interpretation of propositions turns upon the fact that they render certain combinations false or impossible. Take any proposition we please, and claim the widest latitude we like for its two terms, we shall see that their combination as a proposition instantly renders some results impossible. Thus 'All X is Y' denies that there is any X which is not Y. It renders this combination of the terms false, inadmissible, impossible, or whatever else we like to call it[1]. When we come to the case of reasoning we find the same result. Whether we take the narrow course of rejecting only what the premises render impossible, or the more rational course of rejecting whatever those premises do not force us to accept, the same distinction

[1] That is, whereas before the utterance of the proposition this particular combination might have stood on the same footing as all others, it is now at once distinguished from them and declared to be inadmissible.

emerges between what is admissible and what is not, in a word between what we are and are not to hold for true.

(ii) The second kind of test or standard of truth is the ordinary one of science, or rather of practical life generally. It must be admitted that the test is often a very difficult one to apply, and that it is exposed on various sides to considerable metaphysical objections. But it is the only one which can be admitted into Inductive Logic, or into any department where speculation is to lead out into practice. The simplest account of the 'real' concepts, as of the 'true' judgments, on this view, is given by saying that they are those which will ultimately justify themselves in experience. Such a test is of course in many cases a long and troublesome one to carry out, and we may have many difficult questions to discuss before we finally bring it to a really satisfactory termination. Mere clearness of conception is not sufficient. For instance I have as clear a conception of a unicorn as I have of a wombat, in fact, as far as that is concerned, a much clearer conception. My first resort, when there is a demand for their justification, is perhaps to pictures or other casual references, and there I find them both. I may perhaps then appeal to the narratives of travellers, ancient and modern, and there too I find them oth; and this may lead me on to a comparative estimate of ' veracity and accuracy of the respective travellers. Where ⁻annot secure what a schoolmaster would call an 'object-', in other words perceive the thing at first hand for ·s, a long and intricate enquiry may be necessary; but ' appeal wherever possible ought to be to the actual ıs of ourselves or others. Sometimes even this is not nd then we have to express ourselves hypothetically that *if* we were there, wherever the right place may ld find the proper experience corresponding to the

ˑre is still a third standard of truth or reality consideration. We sometimes have occasion to authority which we may happen to accept as e purpose, without going behind our record, so uiring whether this authority itself could be h an ultimate sensible reference as suggested ·m a notion of a centaur with only two legs,

this is, in a perfectly intelligible and reasonable sense of the word, *wrong*; for the only immediate appeal here is to the poets and painters and the others who deal with such an animal; and the only ultimate appeal would have to be made to the early legends and beliefs which these authorities are understood to reproduce. Or if I say that Mr Winkle received his challenge at Bath whilst another corrects me and asserts that the event occurred at Rochester, it is equally clear that my statement is wrong and his is right. Here the only authority, immediate and final, is to be found in a certain book, conformity to which is the test of truth. Such standards as these may be widely various, and the appeal must be decided in each case by the nature of the subject-matter. Sometimes it is to a single book, the author of which has invented the character of the person or the occurrence of the event; sometimes it is to current tradition or ancient legend; sometimes it is to the established conventions or definitions of some class or profession, as when we are dealing with the various creatures which enter into Heraldry. Many of their characteristics admit of perfectly definite statement, and according as we conform to convention or not our statements and descriptions are right or wrong. Some standard there must be whenever the question is raised about our notions and statements being right or wrong; and unless we are talking at random, or inventing a story at first hand, this question is a pertinent one in every case.

Most of the above discussion may perhaps seem to the reader to be rather wide of any mark at which he expects to aim, and as partaking of the nature of needless refinement. This will prove, we hope, in the sequel not to be the case. In a chapter devoted, like this, to Prolegomena, we must be content with a bare indication of its importance, but one illustration may be offered here. In a well-known portion of his *System of Logic* (Bk. I. ch. 8) Mill has discussed the import of Definitions, and has decided that they do not presuppose the 'existence' of the thing defined. As a proof of this he offers the definition of a dragon as a serpent which breathes flame, and draws the conclusion that if we allow the definition to carry with it the existence of the *definitum* we could establish the fact that there *are* serpents which breathe flame. This

discussion has given rise to a relatively large amount of criticism and dispute, and I apprehend that most readers must have found Mill's views, as he himself has expressed them, anything but satisfactory. I am convinced that the matter is really a very simple one, and that the difficulties arise almost entirely from our not distinguishing between the two very different enquiries, viz. (1) the necessity of presupposing *some* test of reality, which is inevitable in every case, and (2) the selection of our particular test in each case, where there is considerable option. When any one proposes a definition of a dragon he is naturally presumed to be appealing to our third test, viz. the artificial or conventional one; and tried by this the definition and the conclusion drawn from it are both unassailable. There *are* dragons to be found—plenty of them —if we seek in the right quarters; and the definition is a fairly sound one. But when we drop the reference to the dragon, by eliminating him as a middle term, and regard the conclusion as a bare statement by itself, that 'certain serpents do breathe flame[1]', we are of course supposed to be appealing to a standard appropriate to serpents simply, viz. the standard of ordinary experience. No error can result from adhering to one standard only, but the hesitation between two naturally produces confusion.

VII. The last of our preliminary postulates arises out of the nature and functions of Language. It may be expressed in the double statement that we must assume that our words have the same determinate meaning in the minds of all who use them; but, inasmuch as it is this meaning which is the

[1] Mill's words are as follows,—

"Out of the definition we may carve the premises of the following syllogism:

A dragon is a thing which breathes flame:
A dragon is a serpent:

From which the conclusion is,

Therefore some serpent or serpents breathe flame."

If, after the explanation offered above, the question be repeated, Does then a definition imply the existence of the thing defined? I should say that the word 'imply' is rather too strong, and should prefer to say that it *suggests* the existence as extremely likely, on the ground that one would not generally take the trouble to define what did not (within its own proper region of course) actually exist.

important thing, we enjoy full liberty to substitute any equivalent rendering so long as the meaning is not interfered with.

(1) In reference to the statement that words must have the same meaning in the minds of all who use them, one has really some difficulty in insisting upon this as a postulate; for it is at once so absolutely necessary for scientific accuracy and yet in practice so obviously untrue. As regards its necessity there can hardly be a doubt; for how are men to communicate their thoughts to each other if the only symbols for their thoughts are differently interpreted? or how are they to retain their thoughts in their own minds if these same symbols have a fluctuating signification? That such a postulate is not confined to Logic needs no pointing out. It is implied in every kind of intercommunication about facts, and only forces itself specially upon our notice in this department, owing to the more than usually accurate use of language which is here required of us. It may be added that such an assumption cannot even be disputed or denied openly, except in a way which actually justifies it; for we cannot know that any one is questioning it unless we take for granted that the expression of his difficulty is to be interpreted in his mind just as it is in our own.

What we are here doing is, in fact, to add on a third order of phenomena to that fundamental duality upon which Logic was shown to be built. Were our language a perfect instrument there would be no occasion to introduce any such distinction as this, for our words would then correspond with absolute flexibility to every modification of the thought which they were intended to convey, and would have no irregularities of their own which could be a source of disturbance. Though the object, the notion, and the term, belong to three different ranks of existence, the two latter should be, to all intents and purposes, one. The utterance of the same word ought always to excite the same notion in all minds familiar with the language, and the same notion (or corresponding notion as it would be more accurate to call it) must be capable of verification by appeal to the same physical objects. The correspondence ought to be complete throughout—or, rather, complete up to a certain point—for, as will be shown immediately, there is a point beyond which the agreement is not demanded. But within the required limits the harmony should be exact. The healthy and

normally-minded speaker should as he speaks translate, or be capable of translating, each distinct term into a distinct notion; and if challenged should be able to give definite reference to the external things corresponding to all these notions.

It was just above remarked that the correspondence between the notion and the thing is only demanded up to a certain point. The limitation implied is an important one in Logic, and will fully occupy us in a future chapter. It suffices here to say that this exception turns upon the distinction between what are called essential and accidental attributes. Suppose, for instance, that two persons stand opposite a tree, and utter the word 'tree' as they perceive the object. That object itself possesses an infinite number of attributes, in the sense that there is no limit to the number of abstractions we may make from it, or the number of relations in which we may view it. Now look at the second element, viz. the mental one. The intuitions and the notions as to the tree which those two persons entertain will consist of elements which are very numerous though falling far short of the infinite variety of the object itself. But there will be an agreement between the two observers up to a certain point, although the differences will be many, according to their capacity, temperament and previous experience. It is not for a moment contended that this should be otherwise,—it would be a dreary world indeed if it were so,—all that is demanded is that a certain number of the more important of the elements of these notions should be the same, and that the two observers should agree as to which these elements are when they use the same name. In technical language they must be supposed to agree as to the *connotation* of the names they use whenever these are general names, and as to the objects denoted whenever these are proper names.

That the agreement we desire, even within these limits, is very far from being attained at present, needs no pointing out. There is a very wide divergence amongst us not only as to what names ought to mean, but also as to what they do mean, and this divergence exists amongst the best informed as well as amongst the ignorant and careless. The conclusion I should draw from this,—though it is doubtful if any logicians have insisted upon it,—is that Logic is in this respect to be regarded as a sort of *applied* science, in the same sense as is Mathematics.

In both cases, in order to make our subject-matter capable of scientific treatment at all, we are forced to make assumptions, as to the simplicity and abstract perfection of our materials, which are not justified in practice; but we know that it is only in so far as they are justified that our conclusions will hold good. There are (as far as we know) no straight lines or perfectly regular surfaces in nature, but when we have to perform a piece of mensuration it is only in so far as we assume that the lines and surfaces with which we deal are perfect that we can apply our formulæ. This state of things gives the science its 'applied' or 'hypothetical' character.

Just so is it in some respects, I apprehend, with Logic. Where our science mainly differs from Mathematics here is, that the existence of this divergence between what theory assumes and what practice exhibits is really one main reason why the study of Logic is found to be serviceable. This may sound paradoxical; but we must remember that, as regards a large part of the subject-matter of Logic, our notions or concepts are so far within our own control that we may modify them and bring them into better accordance with those of our neighbours. We postulate perfect accordance, and find that it does not exist; but, finding this, the remedy is at once suggested, and we do what we can to apply it. Look, for instance, at *Definitions*: if these really were exactly what we profess them to be they would be perfectly useless. We describe them by saying that they unfold the connotation or meaning of a term;—but this must be known already, or it could not be the meaning. We say that being verbal they 'give no information', as real propositions do;—and then what could be their use? The answer is,—we are speaking of course of familiar words of common occurrence, not of comparatively new scientific terms,—that the laying down of a definition really implies an agreement as to the signification; but that attention to the rules of definition, and practice in dealing with them, constitute our best means of attaining towards the realization of such an implication. We know that in fact the few accurate and careful thinkers lead, and the bulk of men follow; and that the notions of the latter are defective and erroneous in a high degree. But no scientific treatment of language as a means,—we are not referring, of course, to its study as an end, in the hands of the comparative

philologist,—would be possible unless we proceed upon the fiction that all men mean the same thing when they employ the same word.

It would not be fair to the traditional treatment of the subject to imply that all reference to such a postulate as this has been entirely neglected. Though not openly announced as a postulate, its existence and necessity are indirectly intimated by a discussion of certain characteristics of concepts, namely their clearness and distinctness. The nature of these characteristics is somewhat laboured over by Hamilton, and will be found discussed (amongst other English writers) by Abp Thomson. A concept is distinct, they tell us, when it can be distinguished from others; and clear when we can distinguish the separate elements of which it is composed. As these technical terms are used by these particular writers, the difference between them does not seem to me to be very easy to establish; for the concept, being nothing but the sum-total of its constituent elements or attributes, I fail to see how one such group can be distinguished from another unless we distinguish the elements of each amongst themselves, and vice versâ[1]. However this may be, the discussion of concepts under these relations must be admitted to be a recognition of the fact that they do not actually exist in our minds as our logical rules must suppose them to exist. We might vary Hamilton's account of the matter slightly by saying that it is a postulate of Logic that all concepts are to be taken as clear, otherwise they could not be consistently held and distinguished from others; and that we all agree in our estimate of these clear concepts, otherwise we

[1] The distinction seems to have been familiarized by Leibnitz (*Meditationes de Cognitione Veritate et Ideis*), and as used by him had a better warrant than it can show in the treatment of some later writers. For one thing he did not, like Hamilton, profess to be speaking of *concepts* only. He applies the distinction to our intuitions or perceptions also, which makes a considerable difference; for these do not, like true concepts, consist mainly of constant attributes, but include all manner of personal, fluctuating, and accidental attributes as well. It is obvious that we might readily enough distinguish between two concrete objects, which happened to be before us, by aid of these latter characteristics, when the notions or concepts entertained of them were very hazily held. In addition to this Leibnitz interpreted the distinction in connection with his doctrine of "petites perceptions", or unconscious mental modifications, which puts the matter in a very different light from that which it derives from the treatment of many other writers.

could not carry on rational communication with each other; and that we couple with these statements the admission that in practice neither of these assumptions really holds true.

(2) The other clause of the general assumption which we have to make about the use of language, is of a rather different kind, and is in fact a postulate in the Euclidian or more usual sense of the word. It is merely a claim to be allowed to do something which we require to do for the purposes of our science. It may be simply expressed by saying that we claim the right to vary our language as we please, provided no substantial change is introduced into the meaning involved.

The legitimacy of such a claim as this arises out of the subordinate position here assigned to Language in comparison with the two other elements which stand in relation to it, viz. the notion and the object. In the scheme of any writers who should,—as Whately indeed avowedly does,—claim language as the immediate and almost the sole subject-matter of Logic: or in the schemes of those who, with the older logicians, assign to language a very prominent place in their treatment, it would be otherwise. Any proposal to change the words employed ought then to be regarded with some suspicion. But with us the postulate seems a perfectly reasonable one. After the discussion in the earlier part of this chapter it will be understood that the only really fundamental subjective elements are the notions we entertain about the objects: the words which stand for these, in spite of their apparent prominence, have really the secondary function of enabling us to determine, to retain, to extend, and to communicate these notions. In other words, whereas in Grammar language is an end in itself, in Logic it is rather a means to an end, though doubtless a most influential one.

Hamilton is one of the few logicians who have recognized the propriety of formally making this claim, though the statement in which he advances it is adapted to the peculiarly subjective aspect of the science as he treats it. He asserts the right to state explicitly in language all that is contained implicitly in thought. I prefer to remind the reader prominently and continually that it is not with thought alone that we are directly concerned, but with the duality consisting of the thought and the object. Moreover the expression 'implicitly

contained' is a supremely awkward one in Logic, since logicians by an overwhelming majority maintain that every conclusion is implicitly contained in the premises, and it is hardly proposed to make *this* substitution under cover of a postulate. None the less however Hamilton deserves the credit of being one of the few to make the formal enouncement of this postulate.

Some of the applications of this principle are obvious enough, and repeated instances of its importance will occur in the course of this work. In the syllogistic processes of the Formal Logic we have perpetually to resort to it. Popular speech is infinitely varied in its admissible forms of assertion and denial, whereas Logic only admits a very limited number of forms,—the traditional system indeed accepts only four typical forms of categorical proposition,—hence very considerable modifications may have to be resorted to in order to throw a given popular statement into its suitable technical expression.

That the practical carrying-out of this liberty of equivalent substitution is a difficult and delicate task in certain cases, must be frankly admitted. It is an illustration indeed of what any sensitive and poetic mind, fond of dwelling on the niceties of language, must regard as the brutality of logical procedure, that any such equivalence of rendering should be considered to be possible. Even as regards terms denoting familiar objects, every one knows what a piece of work it is to find two words which shall not merely be dictionary synonyms, but shall be such that one of them could take the place of the other without alteration of meaning. Even if we can find two which strictly mean the same thing, that is, which apply to exactly the same object or class, there are sure to be differences amongst the many associations which cluster about them and blend with the true meaning. Amongst these the logician roughly pushes his way, cutting off every constituent which seems to him accidental or personal, till he comes down to the residuum which may be regarded as fixed and common to all who use the term. This trite and commonplace remainder may then be regarded as the truly exchangeable medium. It need hardly be said that the task of thus deciding what elements may, and what may not, be spared without harm, is not an easy one.

Similarly in dealing with propositions. The difficulty here

mainly lies in deciding at what point mere substitution of equivalents merges into actual inference. In other words, what change of language is possible here without any advance in the signification? We may start with saying that inference involves the obtaining of a new and distinct proposition; but then in these doubtful cases everything turns upon whether the substance of the proposition has really been changed, or only its verbal expression. Take an example. Most logicians would admit that 'some men are mortal' is a different proposition from 'some mortals are men', but they perhaps would not admit that 'Tully is Cicero' is a different proposition from 'Cicero is Tully'. This subject will turn up for discussion hereafter, when we come to examine into the functions and value of the syllogism. At present I will merely remark that there does seem to me to be a difference between the above two examples, —a difference of meaning, that is, lying below any difference of wording;—for it is not the same thing to think of 'some men' and to qualify these as 'mortal', as to think of 'some mortals' and to qualify these as 'men' or 'human'. On the other hand, when we are dealing with the other proposition as it actually presents itself in the mind, we see that what we are really doing is mentally to call up a certain person, and to assign him two names, and the order in which these two names are imposed is a mere matter of language.

CHAPTER II.

THE FOUNDATIONS OF LOGIC; MORE PARTICULARLY IN THOSE RESPECTS REQUIRED FOR INFERENCE.

SEQUENCES.

THE discussion in the preceding chapter was directed towards the task of what might be described as getting the world ready for the logician to set to work upon it; or rather of getting it ready for that whole group of processes which constitute Science and rational practice, and of which the procedure of the logician is but a part. The position we have reached, so far as external requirements are concerned, is that of an objective world capable of being imbued with order, but not yet necessarily regarded as orderly. We have, in fact, got onward as far as Chaos. The little fragments of consciousness with which the psychologist might be supposed to start were assumed to have been projected outside us, and gradually built up into a multitude of distinct objects possessing attributes which accompany or succeed each other. Not a word however has been said about any *order* amongst these objects. An external world had been constituted, but for anything we had yet seen it might still be a Chaos rather than a Cosmos.

What we now propose to do is to take the further step of imbuing this chaos with order. That is, we are to consider certain narrower and more special assumptions which lie at the foundations of Logic in particular. Owing to their close connection with the subject-matter of this science, we shall have to be more minute in explaining and justifying them than was demanded in the case of such extremely general assumptions as those indicated in the preceding chapter.

Bearing in mind then the general scope of Inductive Logic, what we have to do is to aim at explaining and systematizing

the facts of the world throughout their widest possible extent. But as the overwhelmingly larger proportion of these facts is beyond the range of our immediate observation, this implies the discovery of some kind of order, arrangement, or relation among the facts,—we purposely use various and vague expressions at the outset,—for, without this, we may take it for granted that we could not advance many steps on our path. Whatever else Induction may be, it involves a passage from what has been observed to what has not been observed.

What characteristics then ought we to demand in Nature in order to enable us to effect this step? That the principle which is to justify us must be a very broad one,—in fact one of universal application, if all nature is to be regarded as amenable to inference,—seems obvious. Moreover such a principle must be an *objective* one; that is, it must express some regularity amongst the events and phenomena themselves, for it is these primarily, and not our own thoughts, whose arrangement we want to ascertain.

There will, I presume, be a tolerably general agreement in the answer given, viz. that what we are in search of is the doctrine of *Causation*, in some sense or other. But when we come to put a precise interpretation upon that term of so many significations, we find it no easy task to choose amongst the many which are offered to us. Is what we want the Law of Cause and Effect, in the sense of regular antecedent and consequent, as Mill in common with the majority of the Scotch school would maintain? Is it a small selection of wide physical generalizations,—objective in their application, but derived from subjective necessities, and therefore capable of à priori proof,—such as the Persistence of Force, the ultimate Rhythmic tendency of all motion, and so on, as Mr Herbert Spencer holds? Is it one single principle alike for Deductive or Formal and for Inductive or Material Reasoning, such as what Jevons advanced under the title of the Substitution of Similars? Or is it lastly a mere congeries of subordinate physical generalizations, each of these being derived from its own special branch of science, but incapable of reduction into unity with the others, and therefore insufficient to form the basis of a system of Inductive Logic, as Hamilton and Mansel maintain?

As I cannot altogether agree with any one of these conflicting

views, though each of them contains a certain amount of truth, the most orderly plan seems to be to commence with a short historic sketch of the doctrine of Causation, in so far as Inductive Logic is concerned. I do not for a moment presume to contemplate writing a history of so ancient and varied a conception as that of Cause; nor is it suggested that the analysis which is here given follows accurately the order in which the successive views have been predominantly held. We are confining ourselves entirely to the logical applications of the doctrines in question. It is therefore proposed to do no more than show how some of the various formulæ, of which a selection is given above, have grown by a natural evolution, through the promptings of common sense and the criticisms of the logicians and physicists in their combined efforts to secure a good foundation for our Inductive inferences.

Any logical account of the treatment of Causation is bound by custom to begin with the Aristotelian view of that relation. For many centuries almost every work on Logic contained substantially the same doctrine, viz. that there are four kinds of Cause,—the Efficient, the Material, the Formal and the Final. This account was derived, as need not be said, from the great Authority himself, or rather directly derived from his latin commentator and interpreter Boethius; but for our present purpose it will be best not to go further back than is necessary in order to exhibit the doctrine as it was commonly held just before it began to be influenced by the rise of accurate physical investigation. The current view for the time being is what we want, and for this purpose the version given by such a widely circulated handbook as that of Burgersdyck will best answer our purpose. His brief description of the four recognized kinds of Cause is given in the following terms:—

Quidquid fit ab alio fit: nihil fit a seipso	(Causa Efficiens.)
Nulla res finita potest aliquid ex nihilo producere: ergo datur materia ex qua res	(Causa Materialis.)
fiunt, et forma quæ in materiam introducitur cum res aliquæ generantur	(Causa Formalis.)
Denique nihil agit temere	(Causa Finalis.)

This account is still repeated here and there in modern works or modern reprints. Some of it falls in readily enough

with familiar modes of thought; some of it has been introduced there through the agency and influence of the old logicians. But most of the distinctions which it involves are obstinately hostile or indifferent to the bent of all current thought, whether popular or scientific.

For instance, the distinction between Form and Matter, clear and admissible as it is in certain applications, and so long as we do not wander far from the original physical signification along a metaphorical track, gives rise to endless subtleties when we attempt to generalize its use. We understand exactly what is meant by 'form' in its geometrical application, e.g. the form of a cube in contradistinction to the matter of which it may be constructed. We may also fairly enough speak of the form of a proposition (e.g. the affirmative), or of a reasoning (e.g. the hypothetical), apart from the subject-matter with which they deal; and it is on this ground that we are justified in separating off, and treating apart, what is commonly called Formal Logic. We could even, by a stretch, admit the legitimacy of speaking of the 'form' of the causal relation in general, meaning by this only the characteristics which were considered essential to that relation, say invariability of sequence or whatever else it might be. But given any particular example of causation, say the melting of wax by fire, to determine exactly what is here meant by the form and the matter respectively seems a rather hopeless piece of subtlety. It is only necessary to notice the straits into which a few conservative logicians have been reduced in their attempts to retain and apply the old distinctions in this particular example, in order to be convinced of their futility for any modern requirement.

The Efficient Cause has shown much greater vitality, being earnestly supported by writers no further removed from our day than Reid and Stewart, and indeed by many even at the present time. Brown, one of the best known Scotch opponents of the doctrine, evidently regarded himself as a bold innovator in maintaining that we have no idea of anything in the way of efficiency beyond mere regularity of sequence. The doctrine that there is some element which may be called 'efficiency' has, it must be admitted, a strong foothold in popular belief; for the natural mind rebels against the view that such active agency as we see around us implies nothing more than mere

regularity. But such a doctrine seems none the less inadmissible into Logic, and for this reason. The efficiency, as commonly understood, is a constant accompaniment of every case of genuine causation; and, being such, can serve as a ground of distinction or of inference in no case. Efficient causes are often spoken of as if they were a distinct *kind* of cause; but when we look closer we find that what is meant is that efficiency is an element invariably present in every case, as something to be added on to the mere regularity in order to complete the conception. It may be claimed on behalf of this element that the *quality* of our certainty of inference is thereby altered, but it cannot be claimed that our *range* of inference is widened. We may detect the element, in the melting of the wax; but its introduction here adds no further information, so far as inference is concerned.

Similar remarks apply to the Material Cause. Understanding 'matter' here in the widest possible sense, as equivalent to logical subject-matter, it is obvious that (as Burgersdyck says) it must be always present under some guise, for it is wanted in order to differentiate and individualize the form. And being thus an element which remains present throughout, one fails to perceive how it can be of any help to us in drawing up rules of inference, where distinction between one case and another is essential.

Nor does the last of the four, namely the Final Cause, offer us the prospect of any better help. There seems even a nearer approach to unanimity as to its treatment at the present day;—unanimity, that is, in respect of its admission as a speculative truth, and of its rejection as a ground of distinction or of inference. By this I mean that almost every sound and reasonable Theist must recognize Design as a general truth, and would admit probably that we are able to detect it in the broad tendencies of Nature; but he shrinks more and more from presuming to identify and reckon upon it in the individual cases and special classes which compose the bulk of our inferences. Of course if the use of the term Final Cause is to be extended, contrary to current usage, to embrace *finite* intelligences, these remarks would no longer apply. The intention of an agent like ourselves might often be identified readily enough; and when thus distinguished would commonly

be fitted into its place along with the other antecedents. But as such an interpretation is quite opposed to usage there seems no need to refer any further to it here.

It is easy to see therefore, that, when we compare the old scholastic account of Causation with the modern scientific or logical one, there seems to be very little in common between the two except in respect of some of the terms in use. The difference is shown as much in what is omitted from the former, as in what is introduced into the latter. We accept very little of the former at the present day, for any practical purpose. For instance, we commonly make a clean sweep of the distinction between Form and Matter, on the ground that the area of its distinct and suitable application is so extremely narrow. And as regards Efficiency and Design, though many writers still admit the former, and most writers admit the latter, yet they admit them rather as general speculative truths than as principles which can ever be appealed to when we want to authenticate a fact or establish a generalization.

It is, on the other hand, to an element not included ostensibly amongst the above divisions, and scarcely even suggested by their verbal statement, namely that of *regularity*, that we now attach nearly all the importance. It may seem at first thought strange to those who are only familiar with the modern usage of the term, but it is nevertheless the fact, that hardly any notice whatever is taken of this characteristic in the old treatment of the subject. The doctrine is of course implied in much that is said, for we can hardly speak of causation without such implication, but it is seldom mentioned and never emphasized. It is, I suspect, almost entirely to Hume,—at least so far as the course of English thought is concerned,—that the first definite impulse towards shifting the signification of Cause and Effect from the old track to the new must be assigned. It needs but a reference to his great and popular English predecessor, Locke, to realize the magnitude of this change, and to fix its date. In the Essay Concerning Human Understanding there is a long chapter devoted to the topic of *Power*, that is, to the discussion of Causation under its aspect of Efficiency. It is marked by all its author's customary ingenuity and common sense. But the modern doctrine is conspicuous by its absence. As regards this latter we may say with some confidence that

almost the only reference to it, made by Locke, is an indirect one, in which the regularity of nature is taken for granted rather than expressly asserted. Contrast with this the clear and emphatic declaration of Hume, that, "When it is asked, What is the nature of all our reasonings concerning matter of fact? the proper answer seems to be that they are founded on the relation of Cause and Effect" (Enquiry concerning Human Understanding, Sect. IV.), and again, "All belief of matter of fact or real existence is derived merely from some object, present to the memory or senses, and a customary conjunction between that and some other object. Or, in other words; having found, in many instances, that any two kinds of objects, flame and heat, snow and cold, have always been conjoined together; if flame or snow be presented anew to the senses, the mind is carried by custom to expect heat or cold, and to believe that such a quality does exist, and will discover itself upon a nearer approach." (Sect. V.) I do not suppose that any one familiar with the traditional modes of thought amongst our English philosophers, whatever their school may be, will deny that this is substantially the answer now almost universally given, so far as Logic is concerned; though of course they differ widely in their answer to the succeeding question, namely What is the foundation of our belief in the regularity thus asserted?

What we now propose to do is to trace the natural development of this view; viz. the view that it is *regular sequence* of some kind or other which constitutes the whole logical significance of Causation.

Briefly then we may trace three successive stages in this evolution (so far as the purposes of reasoning are concerned), brought about by the continual attempt to endow this notion of regular sequence with greater precision. There is, firstly, the rude popular view which lends itself to most of the inductive reasoning not only of the savage, but also of the 'plain man', or the uncultivated classes, to this day. There is, secondly, the amendment of this view represented by the logicians and physicists of the type of Hume, Brown, Herschel and Mill. This is substantially, we must insist, the same view as the popular one; though in several respects it marks a great advance in the way of scientific precision. It lends itself to

the bulk of what may be called the careful reasoning of practical life, and to the methods of popular science. And thirdly there is a refinement upon the last, which has been more lately introduced by some thinkers. This view endeavours to guard against certain more or less obvious flaws in the two preceding accounts; but in doing this, or in striving to express the law of sequence with rigid precision, it renders that law suitable only for hypothetical conclusions; in other words renders it useless for positive inductions about matters of fact.

I. In the primitive or popular conception, it must be remarked, no difference is recognized, in respect of their character or importance, between Sequences and Coexistences. Practical considerations being naturally supreme at this stage, nothing is attended to except the fact of one thing being tolerably regularly connected with another, so that the one may be safely taken as a hint to us to look out for the other. There is, of course, a pair of elements recognized,—An A and an x as we may symbolize them,—one of which is a mark of the other; but no great heed is paid here to such distinctions as whether A precedes x, or accompanies it, or even in reality succeeds it, provided it is A that is practically first taken note of. Thus, for instance, the red colour of the strawberry is looked upon as a sign of its being both soft and wholesome, and no essential distinction would be recognized between the nature of one of these intimations and that of the other. But redness and softness are coexistences, in any natural sense of the words, whilst wholesomeness is entirely determined by the subsequent consequences. So again the dull haze of a summer morning might be taken as a sign that it *is* hot and *will be* stormy; and again no heed would be taken to any distinction between the two intimations. Or, if I look out of my window on a summer night and perceive that the street is wet, I may at once infer that it *is* cooler and *has been* raining. The fact is, indeed, that it is no easy matter to distinguish between a sequence and a coexistence in many cases, much of the apparent difference turning upon the conventions of language, which often have a way of regarding more or less distant consequences as present liabilities, and giving them a name as such. The 'wholesomeness' of the fruit seems a case in point here. But even when the consequences are known to be remote, and no attempt is made

to ante-date them by our forms of speech, they are popularly placed upon the same footing with the coexistences so far as our spontaneous inferences are concerned.

Again; no trouble is taken towards making any analysis of the phenomena in order to obtain our A and our x. Or rather, to put it more accurately, A and x, being themselves elements which have been detached from their concrete wholes by a process of abstraction and analysis, no trouble is taken to complete their description by enumerating the various other elements with which they were found associated. Take a simple example. I come into a room and feel that it is very cold: on enquiring the 'cause' I find that the window has been left open. Here we have, in customary logical parlance, our A and our x, our cause and effect. But it is seen at once that each of these is only one element singled out from a multitude of others which accompanied it. Besides the open window, there was the North wind, the size of the room, the exposure in the right direction, perhaps an open door, amongst the positive accompaniments; and an absence of fire amongst the negative ones. So also, turning to the effect, in addition to the coldness of which we took account, there was probably a degree of dampness, of smoke or dirt from outside, and many other elements of which we did not take account.

Now the main characteristic of the popular view, as I apprehend it, is just this; that, where it is dealing with such cases as these, it commonly singles out one[1] antecedent and one consequent, and regards them as signs respectively the one of the other. The reasons for our confining ourselves to such a limited selection as this are not far to seek. As regards the omission from the group of consequents of all but the one element, the reason simply is that we did not happen to be concerned with any but that one. It was the coldness of the room, in the example quoted, not the dampness that we had in view. As regards the omissions from the antecedents the explanation would probably be one of two. Either the neglected elements are so obvious and so necessary that their presence may be taken for granted without express mention; or they are so

[1] We need hardly remind the reader again of what has been so fully described in the last chapter, viz. that the *oneness* of these elements is rendered such, for the most part, by our regarding them and naming them as a unity.

trivial that, for anything they are supposed to contribute towards the result, it was considered that they might be safely rejected.

As this is the first occasion on which we have introduced the use of letter-symbols to represent the various elements of antecedence and consequence, a word of caution as to their employment will be advisable; for experience shows that students may possess great facility in their manipulation in formulæ, but have very faint realization of what sort of phenomena they are intended to mark. Remember then that the sharp distinctions amongst our letter-symbols have nothing at all closely corresponding to them in nature. It is very seldom indeed that the actual phenomena will present so definite an individuality even as the open window and open door in our example above. They will often be found to be nothing more than modifications of one and the same substance or agency; which we can think of and speak of as distinct elements, but which cannot exist as such.

In this symbolic usage groups consisting of a number of letters are commonly introduced: we represent, for instance, *ABCD* as being followed by *xyzw*. It is a usage which will be abundantly familiar to readers of Mill or Jevons. Consider for example what is involved when we say that in such and such a case death was caused by poison. Here the 'death' is a highly complex group of elements reduced to a unity by thought acting through language. We symbolize it by *x*. Similarly with the *A*, or the taking of the poison. This being a definite and voluntary act has a slight degree more of natural distinctiveness about it, though, like the death, its unity is largely the creation of a mental synthesis. This *A* and this *x* represent the two elements which alone are commonly taken into account in the popular estimate. But each of these,—as we shall presently see more particularly,—was really one of a group of elements, and the pair of groups would commonly be symbolized by the relation of *ABCD*...to *xyzw*.... Now what I want here to enforce upon the reader is the comparative artificiality of this letter arrangement, as compared with what nature itself is disposed to show. This *ABCD* and *xyzw* do not stand side by side, so to say, like bottles in a row, or like the actual letters themselves. They do not admit, like these, of separate removal

or transfer. For what are they? They are (to begin with the second term of the pair), the elements of the consequent; such circumstances as the rapidity of the death, its time, place, symptoms, &c., most of these being inextricably involved in what we call the death itself, and only held apart from it by a mental abstraction, which, like the processes of combination, acts through language. Similarly with the elements of the antecedent. The *B*, *C*, *D*, here are such circumstances as the time and place where the poison was swallowed, the state of health of the person, the other food which he took at the time, the remedies he adopted immediately afterwards, and so forth. These are not so much given in a *group* with *A*, as in a concrete *whole*; and we can no more pick out some of them, as we do letters in a row, than we can pick out the sweetness of the taste of an orange and leave that taste behind.

When, therefore, we describe the popular view as selecting *one* element only of the antecedent and consequent, we necessarily imply a certain act of abstraction in such a process. But it is one of those abstractions which the primitive man can well be supposed to undertake, for it acts through, and is well within the limits of, popular speech.

II. Now it is just this popular view from which the logician starts; but he proceeds to trim it into better shape, so as to render it sufficiently explicit and accurate to serve his purpose. In the first place he rejects altogether the coexistences, and confines himself to sequences; a limitation, the grounds and justification of which will occupy our attention in the ensuing chapter. And as regards the sequences he insists upon various refinements of which the two following seem to be the most important:—(1) enumeration of *all* the various elements which comprise the antecedent or cause, or at least all which can possibly be considered relevant; (2) adherence to *closeness* of sequence, that is, to the comparative immediateness of the cause and effect. These improvements represent such an important advance upon the rude popular view, and are so intimately connected with the received methods of Inductive enquiry, that we must examine them in some detail.

(1) We need hardly remind readers of Mill of the importance which he attaches to the enumeration of all the elements of the antecedent. It forms the staple of his exposition of the

causal relation. He criticizes at considerable length the capricious way in which the popular estimate picks out some one circumstance, and regards this as the cause, or at least calls it so. For logical purposes the criticism is quite sound, but there is nevertheless some method in the seeming caprice. The great object of speech is to convey our meaning with the least trouble, and where anything can be reasonably taken for granted, we are naturally apt to omit the direct statement of it. If some of the antecedents can be thus taken for granted, we naturally incline to omit any reference to them. Moreover it must be remembered that the popular interest centres, not in speculation, but in practice. The reason why we look out for a cause is not to gratify any feeling of curiosity, at least not primarily, but because we want to produce some particular effect. Hence every element which can commonly be trusted to supply itself gives us no anxiety, and comes to slip out of our description of the producing circumstances.

Useful as it once was to insist upon the insufficiency of this popular makeshift for a true cause, it seems needless to dwell longer here upon the mere fact that accurate reasoning stands in need of something more than this. If we are to make sure of producing or inferring any particular effect, we must clearly make a point of requiring that all the elements of the antecedent are present, whatever may be the various names,—such as condition, occasion, part-cause, &c.—which they may assume in the popular vocabulary.

(2) The second modification of the popular view will need closer attention. It consists, if one may be allowed the expression, in screwing up the cause and the effect into close juxtaposition; that is, in insisting that the sequence shall be as nearly as possible an immediate one. This is a decided departure from the plain man's way of thinking. Not only does he *not* feel any inducement to crowd up his cause and his effect; he would, on the contrary, find such a juxtaposition highly inconvenient for his purposes. What he desires is some power of prevision, in order that he may take means to avoid the evil and secure the good. With such an end in view it is clear that too close an approximation between the antecedent and consequent would not serve his purpose, for it would not give him any view ahead.

The object,—so far as speculative purposes are concerned,—of securing this close approximation between the elements of the sequence, is obvious. It is done simply in order to secure perfect regularity. A remote sequence can never be a certain one. This is one of the many points in which our common metaphors are apt to mislead. For instance, the stock illustration in Causation is that of a chain where link succeeds link in endless succession. Amongst several other misleading associations connected with this metaphor there seems to be that of certainty irrespective of remoteness. Get a grip at any point of the chain, and, when you give a pull, all the rest will be in tension. But no security of hold at any point of what is commonly called 'a chain of causation' will give us certainty of control of more than a very short length beyond the link which we have in our hands.

The fact is that, in respect of these two logical amendments of the doctrine, the second is intimately connected with the first. The real reason why we are obliged to curtail the sequence consists in the fact that we cannot practically attempt to secure all the elements which constitute the antecedent. This is a point which must be emphatically insisted upon, since it brings into clear light the still essentially practical aspect of this stage of causal discrimination, in spite of the decided advance which it presents beyond the first or merely popular view. We may talk,—as Mill and others do,—about introducing all the antecedents, but this must be understood in a conventional sense. What we really do is to confine ourselves not merely to the elements which we know to be relevant,—itself a rather considerable limitation,—but amongst these we confine ourselves to those which we regard as of sufficient importance. Were it not for this limitation, as will come out more plainly presently when we proceed to discuss the third or final development of the doctrine of Causation, we should not be able to secure that repetition of occurrence which we require in order to apply the sequence we have noticed in the past to some new instance in the future. No two objects or events in Nature are alike in all their details, and therefore if we want to secure repetition we must submit to let go some of the characteristics. In other words, we consent to omit what may be called the trifling or individualizing circumstances in

our antecedent in order to let it get a fair opportunity of repeated occurrence. For practical purposes there is no harm in our doing this, because we really mean nothing else by a trifling circumstance here but such a one as will not soon develop into proportions which would force us to take account of it.

Hence then the necessity of making our causal sequence a tolerably close one. This, or some equivalent resource, is the only means we have of making the sequence at once practically convenient and reasonably trustworthy. Under this safeguard we may safely proceed to omit from consideration, as mere accidents of no account, many circumstances which, if let alone, would develop before long into rather formidable dimensions[1].

The wide prevalence of this particular view of the Causal relation—not merely in our systems of Inductive Logic, but also in the field of popular Science, and in the more careful procedure of common life—makes it extremely important to understand what exactly it asserts, upon what conventions it rests, and what stage of analysis it represents. So far as the recent succession of English thinkers is concerned we might give it the clumsy designation of the Brown-Herschel-Mill view, on the ground that its effective popularization is mainly due to these authors. Brown first formulated it almost in the words adopted by Mill; Herschel showed its significance and value by the rules for scientific discovery which he laid down in his *Discourse*; whilst Mill reduced these rules into more precise logical form, and provided them with the technical designations which have made the Four Methods universally familiar to all students of Logic. We will proceed to analyse this view somewhat more minutely.

Recur for a minute to the example of poison followed by

[1] I find some help here, myself, by drawing an analogy between the physical step of advancing from one group of phenomena (the cause) to the next proximate group of phenomena (the effect), and the purely mathematical step of advancing from the expression $f(x)$ to $f(x+h)$. When we introduce only one term into the expansion of the latter, we must make the step a very slight one; i.e. we must make h very small. As more terms are introduced, the step can be made a little longer, with equal accuracy of result. But in order to secure absolute accuracy, *every* term must be introduced (including the remainder), and this is equivalent in its results to practical inutility.

death. We saw that what the rude thinker does is just to confine his attention to these two elements; and he is so far right, because, in the great majority of the cases with which he is likely to be concerned, the recurrence of that one element in the antecedent will be followed by that of the corresponding one in the consequent. But here step in the advocates of Science and Logic. They insist upon all the antecedents being included, instead of one only, viz. the poison; a condition however which is found on critical cross-examination to reduce itself to the claim that we are to take account of all the important and relevant circumstances. That is, when we observe any particular event, such as the death in question, we now consider that in order to determine the invariable antecedent, and therefore indication of death, it is by no means sufficient to take one element only, but that we ought to have regard to the presence of a certain group of elements. So much is necessary; but so much is also considered sufficient. That is, whenever we have secured the group in question, we feel confident that the event (the effect desired) will invariably follow.

All this may be conveniently represented symbolically, by employing letters to stand for the causes and effects. Thus for the effect we may put a single letter (as we did before), say x, because we still continue to take account of only one element here, but for the cause we should do well to employ a plurality of letters. Suppose we take *ABCDEF*...... for the purpose, the dots being intended to indicate that no enumeration can ever be really complete. Now what the view of Causation at present under discussion assumes is that *some* selection from the above, say *ABC*, will be invariably followed by x; in other words wherever this selection recurs x will recur also, whether or not *D*, *E*, *F* are found there as well. The relation may be expressed thus:—

$$\underbrace{ABCDEF\ldots}_{x} \qquad \underbrace{ABCDEG\ldots}_{x} \qquad \underbrace{ABCEHK\ldots}_{x}$$

Here the letters *D*, *E*, *F*, *G*, *H*, *K*, represent antecedent elements sufficiently important to deserve careful attention, though not relatively effective for the particular purpose in hand; that is, not effective in producing x. The dots may be taken to indicate that in addition to these important elements there are

always an altogether indefinite number of trivial circumstances just powerful enough to act as individualizing agencies, and so to secure a number of repetitions, but which common sense assures us may be neglected for all practical purposes.

In this symbolic notation I have departed slightly from the usual form. Most of the common text-books[1] follow Mill in representing the relation of the antecedent to the consequent thus:—

$$\begin{array}{ccc} ABCD & ACDE & AEFG \\ a & a & a \end{array}$$

It seems to me that this plan is apt to mislead by its suggestion of a simplicity, and a readiness to adapt herself to our wants, on the part of Nature, which she is far from displaying. For one thing since we recognize, and in fact declare, that the cause is always composed of a group of elements, it seems better to indicate this by employing a plurality of letters (ABC). (The constant antecedent, as I have represented it, is of course $\overline{ABC}$; in the other notation it is A alone.) Another defect in the customary rendering is that the employment of corresponding letters in the members of the sequence,—capital letters in the one and small letters in the other,—almost inevitably puts the beginner on a wrong track. It suggests a sort of appropriation of a limited number of distinctly separate causes to a corresponding number of distinct effects, each to each, so that our only task is that of sorting them aright. Now it will be shown in a future chapter that it is quite possible to devise examples in which this happy appropriation really is found to exist, and it will be highly convenient to examine some such example if only for the sake of contrast. But it must be remembered that such suitability is very artificial. Nature commonly presents us with nothing in the least resembling this; and it therefore seems to me better to represent the effect, or elements of the effect, by letters showing no suggestion or intimation of apportionment to such and such parts of the aggregate cause.

As nearly the whole procedure and nomenclature now familiar to students of Inductive Logic spring from this view

[1] **E.g. The Method of Agreement, as represented by Fowler (*Inductive Logic*) and by Jevons (*Familiar Lessons*).**

of Causation, it is well to direct the reader's attention prominently to certain consequences of it. They are partly theoretical, and partly affect the practical rules of Induction commonly laid down.

The first of these consequences is the doctrine of what is commonly called the *Plurality of Causes*; the doctrine, that is, that a given effect may be equally brought about by a variety of different causes, whereas the assignment of a cause necessarily determines the effect. This may need a moment's explanation, since the symbolic representation on the preceding page does not seem in any way to suggest it. This is true, because we were not intending there to illustrate the working of any but a single cause. What is here meant is that although in that particular group of instances $\overline{ABC}$ was present all through, as antecedent to x, this is no bar to a quite distinct antecedent being present in other groups of instances. On a second set of occasions we may find x preceded by $\overline{PQR}$; on a third by $\overline{UVW}$; and so on. It is essential in every case that when we have one of these alternative causes we must necessarily find it followed by x; but there is no necessity that when we have x it should have been preceded by one rather than the other of these alternatives, for any one of them will answer the purpose.

This striking difference in the character respectively of our causes and of our effects deserves notice, since it is generally accepted rather too much as a matter of course. Yet it might well suggest hesitation to those who take no other view of Causation than what we may call the phenomenal one. How can a mere *time* relation result in such a difference as this, according as we look at it from before or from behind? So long as 'efficiency' was admitted as a dominant element in the relation of cause and effect, it might seem not unreasonable to allow an essential distinction between the active and the passive constituent. Now however that we regard the relation as one of sequence only, it may well be asked why it should not be a strictly reciprocal relation.

The answer is not far to seek. The difference between the cause and the effect which produces this difference in their mutual relations, is not one which lies in the facts but arises purely in our way of looking at those facts. Though it is quite

true that the *relation* between Cause and Effect is merely one of time, yet these two elements themselves are determined with a very different degree of rigour respectively. Here again we may revert for a moment to the rude popular view. This, as we saw, picked out one element only on each side, an A and an x, and considered these to be Cause and Effect. Science then stepped in to mend matters, and determined the cause more narrowly by the inclusion of all the important elements; insisting in fact upon $\overline{ABC}$ instead of A alone. But it did not show the same determination when dealing with the effect. It just left that alone, instead of also combining with it some other regular associates, such as yzw.

The grounds of this difference in our estimate and treatment of the members of the causal relation will be examined almost immediately. At present I only wish to call attention to the fact, as it is entirely upon this that the possibility of Plurality of Causes depends. Had we been equally exhaustive in our enumeration of the constituent elements in the aggregate effect as we were in those of the cause, no such plurality would have been possible. Each fresh element included among the consequents excludes some of the alternative possibilities of causation, and the inclusion of all would rigidly confine us to one cause only.

Any simple example will serve to show this. We say, for instance, that death may be brought about in a variety of different ways, and we call all these ways 'causes', and thence deduce the doctrine of Plurality of Causes. The death may be produced by suicide, in any particular case; by disease, and that of various different kinds; by murder; and so forth. But all these alternative suppositions are only rendered possible, because the 'death' is a single element in the sense above described, that is, it has been abstracted from a number of other characterizing circumstances. Had we introduced these other elements or characterizing circumstances, only one of the possible causes would have been admissible. The condition of the organs would have precluded such and such a form of disease; the position of the body and the nature of the wounds would have precluded the alternative of suicide; and so on with each alternative in turn. So clearly is all this recognized whenever it becomes important to take it into consideration, that the

whole procedure in a trial for murder, or in any coroner's court, rests upon the assumption that if we are particular enough in our assignment of the effect there is no possibility left open for any plurality of causes.

It seems clear therefore that the difference in question arises out of a difference of treatment or definition, rather than out of one of fact. But this only throws us back a short step. It still leaves us to face the very pertinent enquiry *why* this difference of definition should have come to pass. The answer to this enquiry, I am convinced, can only be found in the essentially practical character still retained by the view of Causation corresponding to the stage now under review.

Common Language, it must be remembered,—and therefore to a great extent our definitions, when we are dealing with old terms and popular conceptions: in fact our whole spontaneous way of viewing nature,—has come down to us from immemorial antiquity. The logician only takes a term in hand ages after it has been more or less consistently used by rude thinkers and actors, and he therefore finds a great deal of old association clustering inevitably about it. Now how would the conception of a cause, in so far as its regularity of agency is concerned,—and this characteristic we know, though not explicitly stated by the logicians, must always have been tacitly accepted,—be likely first to arise? Surely in the way of practice rather than in that of speculation, and therefore with an eye to the future rather than to the past. What the savage mostly wants to do is to produce something or to avert something, not to account for a thing which has already happened. What interests him is to know how to kill somebody, not to know how somebody has been killed. Of course the past must interest him to some extent, because what has happened once may happen again, but this is a comparatively indirect or remote reference. What holds good of the savage does so also, though to a somewhat less extent, of the great majority of ordinary people: the explanation of the past will naturally be far subordinate in interest to the prediction of the future.

Now these are just the conditions which we find displayed in the popular view of Causation. When we want to *explain* a fact an offer of several alternative solutions affords very little help; but when we want to *produce* a fact a corresponding

redundancy of modes of procedure, so far from being a hindrance, is a distinct advantage. The modern scientific student of early culture vexes his mind to ascertain in which of various possible ways fire was first discovered or produced by man; whether by lightning, by friction of boughs of trees, by sparks from flint chips, or so forth. This attitude is quite correct from the standpoint of speculation. But for those whose only care was how to make a fire when they wanted it, such plurality of causes was all in their favour. Hence, we may assume, the antiquity and persistency of that view of Causation,—or rather of the physical relations to which we of the later times give that name,—which leads as a consequence to the very different treatment respectively of causes and of effects. The inductive logician does not even attempt to alter his formulæ so as to remove this diversity; he contents himself with setting it aside in individual cases, namely when he is dealing with explanation rather than with practice.

We may, for convenience of illustration, slightly vary the statement of our position at this stage. Remembering that the possible additional elements of the effect and of the cause,—those namely which are symbolically expressed by y, z, w, in the former, and by D, E, F, G, in the latter,—are generally not distinct objects or events, but are qualifications or modifications inextricably involved with x and A, we may phrase it as follows. What not only the savage, but also the practical man mostly want, is a *general* result, say the death of an enemy. It does not matter whether the symptoms, i.e. the qualifying circumstances, are those attendant on poison, on a blow from a club, or on witchcraft, provided the death is brought about: but they do desire *certainty* of attainment in respect of this general result. This attitude of mind towards the phenomena is best attained by generalizing the effect and particularizing the cause; that is, by confining the attention to x only in the former, and by insisting upon the introduction of D, E, F, G, &c. in the latter.

III. We now proceed to discuss the final stage, that namely, where speculative interest has got the upper hand, and is prompting us to decide, with full accuracy and perfect indifference, in all directions, past as well as future. We must see what such a determination as this will lead us to.

(1) The first thing we should here proceed to do would be

to introduce all the elements, both those which are antecedent and those which are consequent, in the strictest and completest way possible. Begin with the latter, as they stand most in need of the improvement in this direction. Suppose we really were to insist on being as precise in our demands about these as we were in our demands about the others, we should at once do away with the Plurality of Causes. We should in fact adopt permanently the attitude which we find it convenient to adopt occasionally, as when it happens to become important for us to single out one particular cause amongst the possible alternatives. We saw that this is actually the case in matters of such strong practical importance as a judicial enquiry about an observed case of death. Having done this, the relation would of course become, to whatever degree of precision we had attained, *reciprocal*; that is, given either the cause or the effect the other would be unambiguously determined. We should have fully recognized, and attained to, that complete indifference towards mere time-relation which any doctrine about sequences pure and simple ought properly to exhibit. Our attitude towards the past and the future would then be similar, for speculative considerations would have taken the place of those which are mainly practical.

But here, as elsewhere, a reform once started is not so easily stopped at its first halting place. We began by speaking of including 'all' the antecedents, but we soon perceive that in doing so we were speaking in a rather loose way. Practically we always omit a quantity of determining elements solely on the ground of their comparative insignificance. Take such a simple example as that of dropping a stone to the ground. We say, in accordance with the common expression of the causal relation, that if the stone be dropped again just as it was before, it will fall on the same spot. True; and for most practical purposes the thing can be done readily enough; but if perfect quantitative accuracy were required we should soon find that we had undertaken an impossible task. The stone must be held in exactly the same position as before, for the friction of the air influences its fall; it must be dropped from exactly the same height and over the same spot on the floor; the atmospheric currents, nay the very temperature of the air, must remain unchanged; and so on indefinitely with further

demands, directly those already formulated were assumed to be satisfied.

If it be urged that all this is merely useless subtlety, the retort is simple and conclusive; namely that untold millions of pounds have changed hands in consequence of just these conditions of things. It is simply because we cannot perform the same action over again, or calculate how far we shall fall short of doing so, even when our instrument in hand is purposely made of as accurate a shape as possible, that the roulette and die can be employed for gambling purposes. So impossible is it found to spin a top twice with the same velocity, or to discharge a cube twice from the same position, that the fanatics of the gaming table never attempt to predict results from this side, but put their trust in appeals to statistics and other considerations.

(2) Here comes in a second requirement, namely that of making our sequence an immediate one. As was shown some pages back, these two requirements are closely connected together; and just in proportion as we have enhanced the standard of our demands about the one, so are we forced to be scrupulous about the other. That is, if we will insist upon trying to evaluate every element on both sides of our sequence, we shall find ourselves obliged to help out this attempt by screwing up, so to say, the two components of this sequence,—the so-called antecedent and consequent,—into close contact with each other.

What is meant by this requirement may be made clearer by a slightly different mode of statement. Conceive that there were granted to any one the right of unlimited demand in respect of all the details of the antecedent. We must still insist that no enumeration of the elements, and no accuracy in determining them, possible to a finite mind, could start him so precisely on the same track a second time that he could hope to remain on it permanently. He will infallibly deviate from it sooner or later: and any deviation, however minute, will serve as a basis for disturbing agencies to work upon which will soon aggravate the departure into sensible magnitude. Hence the necessity, if the sequence is to be repeated twice over, that we should limit ourselves to a close proximity between antecedent and consequent. It is as with rifle firing. Fix the gun in a

rigid rest, and do what we will by appeal to all chemical and mechanical resources to secure precise repetition of charge and position, we nevertheless know that there will be a perceptible deviation between the tracks of the two bullets. We could not insure hitting precisely the same spot twice unless we placed the target close against the muzzle of the gun.

The outcome of all this would seem to be an account of the Law of Causation which might be formulated in the following definition:—Given that in any two instances the precise sum-total of antecedents recurs, so will the sum-total of immediate consequents; and conversely.

There seems some interest in working out this view a little more in detail, both as regards its significance and the consequences to which its acceptance leads us. As regards the former there are, it seems, two interpretations open to us which are,—we cannot so much say reasonable, as self-consistent.

(1) For one thing we may really stand to our terms, and insist that we do mean *all* the antecedent elements, stopping short at no degree either of minuteness or remoteness provided any physical connection really exists. Are we, for instance, entitled to claim that the moon and stars shall be in the same position when we drop the stone a second time? Certainly, if we like it. We have shown above that it is very hard to say how far in this direction even practical considerations, as illustrated in gambling, would allow us to go; whilst our theoretic warrant, if we appeal to the Law of Gravitation, is indisputable. Our Causal relation then becomes a fine broad one. It is that suggested by Mansel in his *Prolegomena Logica* (p. 71), but held long before, we are told, by a Mr Muddle (*Peter Simple*, ch. XII.). We say, on this assumption, that if ever, or whenever, the position, course, and details of the whole Universe are repeated at any point, they will necessarily be repeated at every other succeeding point. As a consequence this would involve the theory of an endlessly recurring cycle, after the fashion of a circulating decimal, whatever the length of its period might be.

We need hardly pause to examine this; but in passing by it we may just remark that, even if it were true, there seems some difficulty in showing how we could know it to be so, unless the succession had been absolutely eternal à parte ante.

For the *knowledge* that there had been a cycle before them could not be granted to any of those in the first cycle; for there had been none such: nor therefore to any of those in the cycles which follow them; for all are to be exactly alike.

(2) There is however another interpretation, slightly more reasonable than the above, which might yet admit of being called, in a mathematical sense of the term, perfectly accurate. Suppose that we do not claim to include actually all the antecedents which an appeal to nature, far and near, could bring under observation, but confine ourselves to such as lie at hand, only insisting upon reasonable scientific accuracy in their estimation: does the Law still give any information? It does, but only in case we now confine ourselves by a restriction which we were able just above to dispense with, viz. the rigidly *proximate* character of the sequence. Under this restriction we can, of course, only secure the initial tendency. That is, knowing how events are standing at some assigned moment, we know in what way they will *start*, from that moment onwards. That affords us, remember, no accurate information as to the state of things at any point which lies a finite distance off; it only indicates how things begin indefinitely close to their starting place[1].

Here then are two modifications of that view of Causation which is the final outcome of the assumption that it is to express regularity of sequence. They follow from a rigid interpretation of that assumption. Owing to the comparative looseness with which logicians and metaphysicians are too often in the habit of realizing the significance of really 'necessary sequence' when applied to the phenomena, it becomes important to work out some of its consequences.

For one thing then, it need hardly be pointed out that no such view of Causation could be of any practical utility.

[1] The distinction will be familiar enough to every mathematician. Suppose we have an ordinate y explicitly given in terms of a series of powers or other functions of the abscissa x. We know that if we could take account of the whole series, directly or in any expression which summed it all up, we should have a precise evaluation of y throughout its whole course. If however, for simplicity, we take account of only two or three terms of the series we can still do something to determine y, but with this difference;—that we now only know with accuracy its initial values. That is, we only know how the curve traced out by the extremity of the ordinate y will *start*.

This is obvious in the former of the above two modifications, for a doctrine pitched on such a monstrous scale never could be appealed to by any finite mind, and would be of no use to him if he could appeal to it. Equally so with the other modification. What we all need in order to be guided aright in life is some power of prevision. There must be some reasonable interval between the sign and the thing signified, if the sign is to be of any service to us[1].

In fact we may go further, and say that a perfectly accurate statement of the Causal relation can only be couched in the hypothetical form. *If* the antecedents recur, so will the consequents; but we know they never will recur. We may illustrate this by an analogy. Conceive a man endowed with an infallible memory for any face he has once seen, but who in the multitudinous intercourse of life never succeeds in meeting the same person twice. We can speak with certainty of his power of recognition *if* he succeeded in meeting his man, but such hypothetical power would not avail him much. This consideration will come under our notice in the course of a future chapter, when we shall have to consider in what respect, if any, such a hypothetical regularity differs from actual irregularity, and what sort of additional assumptions are demanded in order to render it of practical avail. At present we are only concerned with it in the form in which it is commonly offered.

Once more. The causal relation, thus stated, becomes absolutely necessary; we cannot conceive its being other than it is. Try, for instance, to picture an infraction of the law on either of the interpretations in question. It is no doubt true that nothing is commoner, especially on the part of those who are unfamiliar with physics and mathematics, than examples intended to illustrate the readiness with which the mind can conceive infractions of what they would term merely physical sequences. For instance we are told to think of a stone dropped twice into the water, but sinking once and floating the other time; of wax held to the fire, melting on one occasion and

[1] We have, remember, no *Integral Calculus* in practical life. In mathematics we may succeed, given an expression which strictly involves only tendencies, i.e. instantaneously successive states, in eliciting from it information as to a result at a discrete interval. But this help fails us in physical problems of such a really concrete nature as those in question.

remaining solid on another, and so on; and we are bidden to contrast the facility of conception of such capricious behaviour here, with the necessary inviolability of such subjective laws as those, say, of mathematical axioms.

I cannot but think that the possibility in the former case only arises from weakness or slovenliness of thought; that it springs from the fact that we do not realize the details clearly and insist upon introducing them all; and that in fact, with similar licence, we might postulate infractions of laws which the writers in question would undoubtedly maintain to be absolutely necessary. For instance, fallacies are notoriously possible even in Formal Logic; that is, owing to slovenliness of thought, or to momentary breach of continuity of attention, we succeed in reaching a result which, if we had steadily thought our way through, step by step, we could not possibly have reached. So in Arithmetic: whenever we make a blunder in addition we can be shown to have gone through a verbal or symbolic process which, if consciously reproduced, would have been seen to involve our making two and three, say, equal to six. So with the stone or the wax. The possibility of picturing to ourselves diverse consequences only arises from the fact that we are omitting a quantity of the details which really combine to make up the concrete instance in question. We may illustrate the distinction by such an example as the following. It is easy enough to conceive two curves drawn on paper, absolutely alike in all respects up to a certain point, but from that point diverging and assuming different forms. They might begin as equal segments of the same or equal circles, and then whilst one continues to follow the circumference the other might go off in the tangent. It is easy enough to do this in respect of the abstract lines. But even here, it seems to me, if we fill in the details by considering the concrete circumstances under which such lines could be produced, the possibility of such discontinuity disappears. The simplest way of rendering such an example concrete would be to suppose the paths to be traced out by moving bodies restrained by threads; that in one case the thread snaps so that the body flies off in a tangent, whilst in the other it continues to restrain the body in a circle. The mere statement of the example in these terms shows that we were not presupposing exactly the same antecedents in the two

cases. And the same explanation seems to apply in the case of every similar example which we can picture to ourselves.

The general conclusion which I deduce from all this is that any attempt to over-refine the expression of the Causal relation necessarily results in rendering it useless for any purposes of inference. Couch it in a perfectly complete and accurate form, and you make it at once hypothetical, and the statement of what is to all intents and purposes a mere identity. For purposes of Inductive Logic, therefore, we may regard the second,—or 'Brown-Mill',—statement of the relation to be the most serviceable. Where I differ from these writers, and in fact from the majority of those who have treated of the subject, is in regarding the statement in question as being essentially a practical one, which does not aim at scientific rigour; as being, in fact, nothing more than a decided improvement of the primitive or popular conceptions on the subject.

That something of this sort is the necessary outcome of the above attempts at refinement has been admitted, explicitly or implicitly, by several recent writers.

For instance, Clerk Maxwell, with that clear and profound insight which he shows into all questions of first principles in Physical Science, has had occasion (*Matter and Motion*, p. 20) to notice the maxim that "the same causes will always produce the same effects." After stating briefly that no event in strictness ever recurs, he says that "what is really meant [i.e. what should be meant] is that if the causes differ only as regards the absolute time or the absolute place at which the event occurs, so likewise will the effects":—a formula, it need not be pointed out, which is perfectly useless for any purpose of inductive inference. In fact this appears to be an expression of that popular view of Time and Space, which was held by Newton and Locke, and probably by most astronomers, in accordance with which these entities are regarded as of infinite duration and extent and as existing, without contents, prior to the insertion in them of the material Universe. And what this explanation asserts is that no variation in the orderly sequences of the world would be produced by any arbitrary change in the place where, or the time when, the whole performance commenced.

Jevons, again, in some of his interpretations of his principle

of the Substitution of Similars[1], so explains it as to imply that only absolute repetitions in every detail ought to count. Thus (*Principles of Science,* p. 238) quoting the remark of Euler that "although he had never made trial of the stones which compose the Church of Magdeburg, yet he had not the least doubt that all of them were heavy..." he goes on to say that "the belief ought not to amount to certainty until the experiment has been tried, and in the mean time a slight amount of uncertainty enters, because we cannot be sure that the stones of the Magdeburg Church resemble other stones in all their properties."

The same view I understand to have been held by G. H. Lewes, when, in a passage much too long to quote or criticize here, he came to the conclusion that "the true expression of Nature's Uniformity" is "the assertion of identity under identical conditions: whatever is, *is* and *will be,* so long as the conditions are *unchanged*: and this is not an assumption but an identical proposition." (*Problems,* II. 99.)

On the whole therefore it seems decidedly preferable, for the purposes of practical inference,—the special function of Inductive Logic,—to take our stand on the intermediate interpretation of the formula of Causation; rather than to attempt to refine it into a needless and merely hypothetical condition of accuracy.

[1] There seems to me to be a frequent ambiguity in his interpretation of this term. When using it, he generally takes in the true sense of *similarity*, for then only can we secure repetitions. But when defining it, he often takes it in the sense of *identity*; and maintains that since we can never obtain this we ought never to claim certainty. His particular example above seems to me a reduction to absurdity. For so "certain" are we that every stone is heavy that, if we did try the experiment and found it fail, we should simply at the time postulate hallucination, trickery, or defect in the balance; and next day resume, if it had been shaken, our customary belief. Surely a single direct experiment would not, and ought not to, convert uncertainty into certainty on such a point.

CHAPTER III.

THE FOUNDATIONS OF LOGIC.

Coexistences.

In the last chapter we discussed the results which would follow from a systematic attempt to refine upon the common statements of the Law of Causation, with a view to rendering its expression absolutely precise. It was found that this could be done; though not without the alternative of either couching the expression in a hypothetical and therefore impracticable form, or of being driven to a merely verbal or identical formula. So far we were dealing with Laws of Sequence; we have now to turn to the discussion of Laws of Coexistence, and see whether any better success will attend us in dealing with them. Here, as there, the motive of the reform would of course be to establish a rigid objective uniformity among phenomena, with a view to drawing inferences.

It is, I think, commonly assumed, and the opinion had the deliberate sanction of J. S. Mill, that the two kinds of uniformity,—those, namely, of sequence and of coexistence, —stand upon a totally different footing. The latter, it is held, are essentially inferior to the former in respect of their certainty and their generality. There is "one great deficiency which precludes the application to the ultimate uniformities of coexistence, of a system of rigorous scientific induction, such as the uniformities in the succession of phenomena have been found to admit of. The basis of such a system is wanting: there is no general axiom, standing in the same relation to the uniformities of coexistence as the law of causation does to those of succession" (Mill's *Logic* II. 115). And there can be no doubt that the ordinary treatment of Causation by logicians and metaphysicians, widely as it differs in many important respects from the System of Logic here quoted, recognizes the same distinction.

The criticism to which the statement of the Causal relation was subjected in the last chapter may have prepared the reader to doubt whether this relation can claim any such decided superiority in these respects, and this suspicion will, perhaps, be confirmed by a careful examination of the real nature of coexistences among concrete phenomena.

I. We will start, as before, with that early stage in which the crude and simple forms of thinking display themselves in the mind of the plain man. We have already seen that at this stage no difference whatever seems to be recognized between the two classes of regularity. Whether in respect of the frequency with which they are appealed to, or of the degree of confidence which is felt in the appeal, it would be hard to say which of the two plays the more important part in rudimentary speculation and in the conduct of practical life. We look at a fruit, for instance, and observing that it is green, conclude that it is sour and that it will give us a pain in the stomach if we eat much of it; but we make no account of the difference, as regards inference, between the coexistent property and the successive one. We look at the fruit-tree, and noticing that the leaves look yellow we conclude that it has been planted in poor soil, that its fruit is weakly and without flavour, and that if left where it is it will soon die; and again these conclusions, past, present, and future, are not referred to distinct classes.

It is obvious that such examples as these belong to what may be called the most elementary stage of physical reasoning; to that, namely, in which we isolate certain objects, properties, or events, and regard one of these as a sign of the other. The pair of elements thus selected to constitute a regular connection,—whether this be one of succession or of coexistence,—are but two out of very many, most of which for one reason or another had been disregarded. Possibly we were ignorant of the others; possibly we had grounds for concluding that they were insignificant in their influence; possibly they were so obvious and so generally influential that their presence was taken for granted. Anyhow, whatever our grounds might be, we selected some one element from each of a pair of groups, and considering their constant connection to be tolerably sure, regarded one as a sign of the other.

II. The next stage,—that of reformed popular thought, or merely qualitative, as distinguished from quantitative scientific inference,—is somewhat more concrete in its nature. In the case of the sequences, we encountered this stage when we resolved to be more accurate and explicit in determining the phenomena, and accordingly found it necessary to insist upon *all* the relevant circumstances in the antecedent. It is not that we purposely aimed at being more concrete, but that from the nature of the case the putting together of a number of elementary antecedents, each of which by itself represents somewhat of an abstraction, necessarily constituted a more concrete phenomenon. We thus got the 'cause' as understood by logicians of the school of Brown and Mill; namely the 'sum total of invariable and unconditional antecedents.' We learned, for instance, that the cause of the unwholesomeness of the fruit consists in various concurrent circumstances in the fruit itself and in our digestive organs, and that, if we take account of all these, we necessarily secure what we call the unwholesome consequences. The insertion of all these antecedents gave, as we saw, a great advance in respect of certainty; raising the character of the inference from the level of the merely practical to that of the fairly scientific.

The question now arises whether by a similar procedure we can effect a like improvement upon our rude estimate of coexistences. The two kinds of inference started almost indistinguishably from the same level: will they both admit of the same refinement? Mill, as we saw in the quotation above, held that they stood on a radically different foundation; that there was, so to say, a definite failure on the part of Nature to furnish the materials for the desired uniformity in so far as coexistences are concerned; that she had not helped us here as she had with her sequences. She did not, speaking in the language of metaphor, make the warp and the woof in the texture of the phenomenal world of equal strength. Longitudinally, or down the stream of time, the fibres are long and tough, but laterally they are few and feeble. In the one direction the web will bear a heavy strain, whilst in the other we can place but feeble trust in it. That Mill was right in drawing some distinction seems certain; but I feel equally confident that he was wrong, or at any rate misleading, in his

explanation of the ground on which it rests. A little consideration will show that the distinction really comes to this: that when the *time* variable is omitted, as it necessarily is in the case of coexistences, it becomes mere tautology to talk of introducing *all* the elements. We proceed to explain.

When the constituent elements in the primitive stage of inference,—call them A and x,—are *successive*, it is easy to insist upon supplementing A by the introduction of all the other simultaneous elements which formed the total antecedent. The worst that could follow from such a refinement was to render the relation practically unrealisable, and to compel us to couch it in the form of a hypothesis: but we found that this hypothesis, though useless, was not quite unmeaning. When however the constituent elements are *simultaneous*, it is idle to propose to take the same steps. The coexistent element x *is* one of the group, and therefore to introduce all the co-factors of A does not entail x as a consequence (as above), but simply finds it where we have that moment put it. The fact is that when we are dealing with sequences we are employing a second order of variables, viz. time, which is not available in the case of coexistences. It would be as unreasonable to attempt to carry out the same processes in the field of the latter which we can effect in that of the former as it would be to endeavour to construct problems of solid geometry when we were confined to space of two dimensions.

It sounds, no doubt, on first hearing, as if the two statements, 'A has been followed by x on this occasion, therefore it will be followed again': 'A has been accompanied by x on this occasion, therefore it will be accompanied again', were so closely analogous that they would admit step by step of the same improvements in the way of rendering them more full and precise. But we can easily see that whereas, in the first case, the complete inclusion of all the accompaniments of A, or in other words of all the antecedents of x, does not actually include x but only compels it to follow, the complete inclusion in the second case of all the accompaniments of A does include x, and therefore results in a mere verbal statement.

It seems clear therefore that the precise plan of refinement

adopted in the case of sequences will not answer in that of coexistences. What then is the nearest approach to such a complete enumeration of particulars which we can permit without falling into a mere identity? It would seem that the formula would have to be framed as follows: If all the coexistent elements, *except one*,—viz. the one which occupies the place corresponding to that of effect,—be repeated, then this one also will necessarily be secured. We have found A on one occasion accompanied by x: if A and all the other elements which coexisted with it on that occasion, except x, be found together again, then we know that x also will really have been secured along with them.

Such a statement as this is clearly significant, for it may give us real information: and it does not seem a necessary truth, for we may fairly appeal to experience to justify or correct it. But on the other hand it does not seem to be of much service in inductive investigation: in fact, when over-refined, these Laws of Coexistence seem of distinctly less value than those of Sequence when similarly reformed.

For instance, one very serious difficulty meets us the moment we attempt to interpret and apply such a formula. It arises out of the necessity that may be imposed upon us, when we begin to talk about 'all the antecedents except one', of undertaking to trace a boundary line between the various attributes involved, and of deciding to some extent as to their identity and individuality. We have prominently brought before us here the fact, so greatly underestimated in the formal treatment of most logical works, that those elements which we are apt to regard as separate antecedents, isolating them and representing them by means of letters, are largely the results of our own more or less artificial construction by abstraction. There is nothing strictly corresponding to them in Nature. It may be remarked that this particular difficulty could be evaded when we were dealing with *sequences*; for when all the elements without exception were to be included any difficulty which might exist as to their mutual boundaries need not enter into the account. If I were to say 'all the countries of Europe are Christian', disputable questions as to their mutual frontiers may be avoided. But if I say, 'all the countries of Europe, except Russia and Greece, belong to the

Western Church', any doubts, if such exist, as to the limits of these countries, might begin to assume importance.

It is quite true that by looking at the matter in a somewhat popular way we can evade most of this difficulty. And as this degree of approximation to accuracy corresponds to what we have called the Brown-Mill account of the Law of Sequence, we will begin with it. Take this case. I observe in a fruit (say a pear) a peculiar shape and smell, a certain size and colour, and so forth. Do I not feel quite confident that along with these will be found a certain peculiar taste? or, if this characteristic be considered to be more in the way of a sequence, do not I feel confident that inside the fruit will be found some pips of a determinate shape and colour? Or again, looking out of a window which commands part of a field in some English town, I see a man with a cricket-bat standing before three stumps and in attitude to strike. Do not I feel almost as certain that some twenty-two yards in front of him, if I could see the spot, I should perceive another man with what is called a cricket-ball in his hand, preparing to bowl it? Again: I see a man drinking out of a cup at breakfast, and am told that there is coffee in his cup: I feel a strong suspicion that 'coexisting' with that coffee would be found sugar and milk.

The mere indication of the above examples will serve to show two things. In the first case they remind us that certain kinds of coexistence when thus interpreted may afford ground for full rational confidence. No one, for instance, would feel the slightest difference between the confidence with which he would anticipate the repetition of the same consequences if he ate two such pears, and that with which he would anticipate that they would display the same general appearance if cut open. But in the second example above we are reminded that the degree of confidence we may possess will vary very widely in different cases. This will best be seen by a brief enumeration, which will be attempted presently, of the principal classes of such Uniformities. It is enough now to say that, whereas at this same general stage of refinement, the Sequences seemed nearly all capable of yielding trustworthy inferences for practical purposes, the Coexistences are many of them very far indeed from coming up to a corresponding standard.

III. Now see what can be done by giving those last

touches of refinement which rendered the Law of Causation so perfectly accurate and so perfectly useless. When we attempt to follow in the same path of improvement we find the difficulty indicated above to be a very serious one. Consider, for instance, the hardness, smoothness, greenness, &c. of a fruit. It is highly probable, from what we know of the constitution of matter, that all these properties spring from the ultimate molecular constitution of the body: so that it is not merely impossible actually to detach one of them from the others, but impossible even to say with precision what we mean by the proposal to do so. We shall see this better if we take a simple substance instead of an organic product. Consider then the colour, weight, toughness, conductivity, &c. of gold. There can be no doubt that these 'coexistent attributes' are the effects, some proximate, some decidedly remote, of the molecular structure of the body, so that we cannot suppose ourselves to make exception of the toughness, say, leaving the other attributes out of the question.

When, indeed, we do insist on penetrating to the bottom, and take our stand upon the actual structure of the body, then it becomes highly probable that we obtain a necessary (and useless) formula closely corresponding to that of Sequence. At least this seems to be the case in regard to those physical properties about which we know the most, and there are many analogies in favour of extending this view to all the others. For instance, in the case of Static Pressure, what is called 'Stress',—i.e. mutual action and reaction,—is so absolutely determinate that given one element the other is necessarily given with it. Suppose one brick resting amongst a heap of others: then if all the pressures experienced by all of them, "except that one", are rigidly assigned, we have assigned the pressure experienced by that one also; for the pressures exerted *on* it are identical in magnitude with those exerted *by* it. They comprise the other side of the pair of equal and opposite forces which we call a stress. Similarly with gravity, or supposed action at a distance. If I know exactly all the attractions exerted by all other bodies upon, say, my inkstand, then I know the attractions which it exerts upon all those.

There are many reasons for extending these considerations in all other directions; and, if so, we should be led to a conclusion closely resembling the final one of the last chapter. It

would seem that a really complete determination of all the interacting forces, at any given time and place, when we try to except those exerted by one object, would determine all the forces which then and there act upon that object itself. In other words; if we revert to our letter-symbols, and suppose that these stand, not for the somewhat remote and loosely apprehended 'properties' of a body, but for the underlying constitution and forces, then and there acting, it is tolerably certain that if A, B, C, D,...(completely enumerated) have once been accompanied by x, then they will necessarily be so accompanied on any future occasion.

We have examined the nature of Coexistences, in this comparison of them with Sequences, to what some may think a needless degree of minuteness. But it is necessary to come to a clear understanding about them, owing to the prevalence of the belief that the two orders of regularity stand upon a radically different footing. As indicated above, we must not admit so deep a distinction. Step by step the two orders of occurrences seem to run closely parallel to each other. In the first or elementary stage, popular thought leans upon one of these classes quite as often and with the same confidence which it feels about the other. And when we commence our reforms of this procedure, still keeping to the popular standpoint and speaking as if the sensible properties or attributes were the elements of the observed coexistence, we obtain a formula of much about the same cogency and value as helped us before. Where the difference is found is not in the characteristics displayed when the relation is at its best, but rather in the fact that in its wide range it includes a large number of coincidences of essentially the same kind but where the cogency is much less; and this hinders us from appealing to it without discrimination. And when we improve our formulæ to the utmost conceivable degree of completeness and refinement they still run parallel with each other. We are obliged in each case to diverge into the language of hypothesis, and to say what would happen *if* such a contingency should occur; but under the check imposed by such hypothesis we speak with accuracy and complete confidence. The whole universe is assigned with the assignment of any part of it. The book now lying by my side on the table is not less, in its present state and position, an

outcome of a long train of past events, and a point of departure for a similar train of future events, than it is, if we choose to make it such, a centre of connection with a surrounding circle of present events. If we consider ourselves forced to regard it as linked before and behind to an "iron chain of causation" of endless duration, we must also regard it as being linked to every object at present in existence with precisely the same stringency and completeness. As Leibnitz says, not only "Le présent est gros de l'avenir; le futur se pourrait lire dans le passé", but also, "l'éloigné est exprimé par le prochain."

IV. As above remarked we practically make large resort, in our inferences, to various kinds of coexistences, and therefore it will be well to see what the principal kinds are. The following seem the most important.

(1) Natural Substances. Here the different attributes are, in a perfectly reasonable and intelligible sense, coexistent. The colour, the weight, the electric and thermal conductivity of gold, for instance, appear to have come into existence at the same time, and to continue to coexist together throughout. Wherever we find two or more,—or, even, if we estimate with rigid quantitative accuracy,—any one of these attributes, there, and there only, do we find all the others.

But the mere statement of such an instance reminds us of the conventional or practical standpoint which we are forced to occupy when we thus speak of coexistent attributes. No one at the present day would seriously adopt that early attitude towards these groups of qualities which we find, for example, in Bacon. Both in his express language, and still more in the rules he proposes for the investigation of Nature, he seems to have regarded a substance as analysable into a certain finite number of "simple natures". Each of these had its *form*, which we might succeed in isolating and producing at will. There was the form of ductility, of yellowness, of weight and so forth. Group all these together, and we have the gold or something precisely equivalent to it:—"If a man can make a metal that hath all these properties, let men dispute whether it be gold or no[1]." And his various tables and practical rules are all devised for the carrying out of this conception.

[1] *Natural History*, § 328.

We need hardly dwell upon the shortcomings of such a conception. We do not indeed know much about the atomic constitution of bodies, but all that we do know appears to suggest that the concurrent attributes arise from the mutual arrangement and motion of the constituent molecules: that, for instance, the colour, and the kind and degree of smoothness and toughness presented by the gold, are results of the way in which the molecules are packed together. The qualities are not, as on Bacon's view, put together as ingredients are inserted to constitute and flavour a dish, but are different aspects of one and the same central arrangement. If this be so, it is scarcely correct to seek the coexistence in the substance itself, regarded objectively; it is rather to be sought on the subjective side. That is to say, we have simultaneous groups of sensations or modes of realizing external things, so that the same substance acts upon us in a variety of different ways, and gives us the appearance of an independent juxtaposition of objective qualities. Thus, as regards simple sensations, the yellow colour and the weight are presented to us as two distinct qualities which exist side by side in the gold. Again, the ductility and malleability are decidedly complex syntheses of sensations, the operations implied in which involve some time for their performance; but they again are relegated to their place amongst the simple attributes, the aggregate of which constitutes the gold, according to Bacon, and characterises it according to us all.

From the practical standpoint, however, which we are now occupying, such an analysis as the above is needless. We are quite ready to admit that, from the common point of view, which is really the only one from which we can observe and describe things in a generally intelligible way for logical purposes, every natural substance contains a group of coexistent attributes. The practical difficulty does not consist in objectifying them,—we can hardly do otherwise without an effort,—it shows itself rather when we attempt to say what belongs to one of these attributes and what belongs to another, in other words to draw the boundaries between them.

(2) The next important class is presented in the somewhat similar field of natural species or classes, such as we find in Zoology and Botany. Every species of animal and plant has

many representatives, and these resemble each other closely in all essential points. The colour, the smell, the taste, of the peach: the speed, the size, the plumage, of the swallow: and so on, remain for all practical purposes the same, whatever specimen we may happen to select. Mill, as we all know, writing in pre-Darwinian days, greatly overrated the distinctness and the ultimate or primitive character of these various attributes. He introduced the technical term of 'natural kinds' to express such classes as these, as well as those considered above, putting them both on much the same footing in respect of natural distinctness and permanence:—it may be remarked, in passing, that this acceptance of the doctrine of the fixity of species is rather significantly selected by Whewell as one of the few points in Mill's treatment which he considered as deserving of praise[1]. Mill regarded these groups of attributes characteristic of a natural kind as being, in the strictly technical sense of the term, *uncaused*. All that we could do when dealing with them was to postulate that they had come into existence together in the first members of the species which had appeared upon the earth. It was held that the continued propagations from the first pair were to be considered as being rather a perpetuation of that primitive collocation of attributes than an entirely fresh appearance of new members. In fact all the aggregate of successive living beings which constituted one of these natural kinds might, for logical purposes, be put upon much the same footing as the various specimens of the same mineral which exist upon earth.

As regards this view it is really no parody, but a simple illustration, to say that in answer to the question, Why are boiled lobsters red? we may reply, This is a case of sequence and therefore a cause can be assigned; whereas in answer to the question, Why are live lobsters black? we must answer, This is a case of coexistence in a natural kind, and is therefore

[1] Mill, it will be remembered, was a keen botanist of the old or systematic type, and fond of herborizing. Probably his only familiarity, at first hand, with any form of natural science lay in this direction. Experience has shown that some of the most determined opponents of Darwinism were found amongst men with this particular training.

In regard to the statement that Mill regarded the properties of kinds as uncaused, it should in fairness be added that he modified his view in the later editions of his Logic.

uncaused. In the former case there is a *change*, and we have given a sufficient solution when we have assigned its cause or the group of unconditional antecedents which preceded it, e.g. the character of the lobster's shell, and the temperature of the water. In the latter case there is no change, so far as the mature lobsters are concerned, and therefore no 'cause'. The black colour is one of the many independent attributes which have been put together as constitutive of that natural kind.

We shall have to revert to such coexistences as these again in a future chapter. When we come to the subject of the 'Explanation' of laws and uniformities, we shall discuss the question as to the origin of these groups of uniformities, and the possibility of reducing them if not to unity at least to the smallest number of independent properties which could serve as a basis for deducing the others.

(3) The two former subdivisions may be called 'natural', in the sense that these coexistences are presented to us by nature, human powers having comparatively little effect in introducing modifications. In the case of simple substances we may be said to have no such power at all. Even if we can alter some one property, which we cannot always do, we cannot help altering all the others at the same time. In the case of natural species, though we have long known that much can be done by persistent efforts aiming always in the same direction, and though we are beginning to recognize that much more than this can be done when the influences are continued through enormous intervals of time, yet so far as short intervals are concerned we are practically powerless.

In marked contrast with this stands a third class of more or less conventional actions, which present the same kind of regularity, though in a much less degree. In the proceedings of a law court, in the series of actions which constitute a coronation, in the positions and attitudes of the players in any game, we may find a group of coexistences presenting great regularity. When we consider how large a portion of our daily life and thought is devoted to considerations in which conventions of this sort play a principal part; and how confidently we infer that where such and such things are found, or such and such actions are being performed, there will other things and actions simultaneously present themselves, it will be absurd to neglect

this class of uniformity on the ground of its unscientific character.

It need hardly be pointed out that there is no great difficulty in attempting to analyze the ground of the unity which underlies such uniformities. We do not require a soul or vital principle or substance of any kind to effect this. We need only start with a few coexistences of a common sort, such as are furnished by the tastes, desires, and capacities of men. Combine with this the wish to secure the same general end on different occasions, and we already have the basis of a class of regular coexistences. Then add on the natural inertia or imitative disposition, and the distinct advantages secured in many ways by exact repetition, and we easily get that stereotyped group of properties which really presents many points of resemblance to a Natural Kind.

Groups of this sort correspond for the most part to those which Locke distinguished under the name of Mixed Modes, and between which and Substances he made so sharp a distinction, on the ground that the Mixed Modes are our own arbitrary institution, being 'put together by men for their own purposes'. This seems to overrate the openings which lie before us for caprice in the selection of groups and the consequent imposition of names upon them[1]. Take the extremest limit of artificiality, offered by popular games. That men can decide for themselves according to what rules cricket shall be played, is obvious; but inasmuch as it is played according to the same rules year after year all the world over, the result is to place it, to other persons, namely to the bulk of mankind, in somewhat the same position as that occupied by any natural object.

Still more is this the case when we are dealing with the actions of men in primitive times, or with those comparatively simple and widespread social phenomena which give but little opening to mere arbitrary choice. Much of our power of interpreting the past depends upon the assumption that the common practices of men, if one may use the language of the farmer

[1] The groups of phenomena known as 'General Election' and 'Cyclonic Disturbance' are, respectively, entirely within human control and entirely outside it. But to the purely logical eye they stand side by side as presenting groups of coexistences, which furnish ground for inference.

and gardener, 'breed true'; that is, that the same groups of attributes will continue to recur again and again over large tracts of time and space. The agents could, of course, if they chose, introduce a capricious irregularity in many of these cases; but so long as from sluggishness, or imitativeness, or from proved convenience, they do not do so, so long will the uniformities persist and demand recognition.

The above are, perhaps, the only groups of coexistences sufficiently important and widespread to deserve notice in such a brief sketch as this. That is, they are the principal groups of a *concrete* character. They necessarily demand a certain amount of analysis, for the recognition and distinction of any attribute demands this; but they do not demand more than is implied in any use of the common language of life. They stand, as has been repeatedly pointed out, on the stage, not of advanced science but, of merely practical requirement. The A, B, C which we regard as a group of coexistent attributes present themselves, so to say, with a good quantity of flesh and blood upon them, rather than as merely anatomical outlines.

(4) As soon, however, as we determine to regard the A, B, C, the coexistent attributes, as being more distinctly abstract in their character, we find an opening to a very extensive and rigidly accurate set of coexistences of a new description. These are, we need not say, the data of Geometry, with all its attendant axioms and theorems. The raindrops that we examine present concurrent attributes of coldness, softness, and moisture, &c., and these when put together constitute the drop almost in its entirety. But if we take the raindrop, and by effort of abstraction isolate everything but its geometrical form, we find that this mere form will by itself give rise to an immensely extensive set of coexistences. The spherical form presents the attributes of perfect uniformity of shape all over, and maximum capacity within a given surface area. These may be considered 'coexistent attributes' in the sense of the term which we have used above; and to these we may add, if we care to do so, all the other characteristic qualities of a sphere which geometers have yet discovered.

Here, as above, the reader will understand that we are merely making a preliminary enumeration. We are not con-

cerned, at this point, with enquiries into the nature and origin of mathematical truths; all that we have to do at this descriptive stage is to direct attention to their existence as a very important class of coexistences, which furnish frequent and confident grounds of inference. If, having paced the sides of a triangular field, I find that one side is the longest I may feel sure that the opposite angle is the largest; just as when I have smelt an orange I know what sort of taste will accompany that smell. In both cases alike we are trusting to the simultaneous existence of certain attributes, and in neither case do we make any appeal to causation in its ordinary logical sense of regular sequence.

V. So much for the directions in which available coexistences are mostly to be found. The next thing which deserves enquiry is the extent of the area over which they can be found to prevail.

The question has sometimes been put in this way, Are there any universal coexistences?—where it is intended to compare them with sequences, in respect of which latter it is considered that universality may really be detected. This comparison in the way in which it is sometimes made does not seem quite fairly expressed. If it be intended to ask, Is there any universal *law* or *formula* of coexistence? we have already seen that this may be answered in the affirmative (at least in a hypothetical way) in the case of both orders of uniformity. If it be intended to ask, Is there any example which can be advanced of a *concrete* kind of coexistence which is really universal? then I should say that there is not; nor, for that matter, could we find one in the case of sequences.

It could not, in fact, be otherwise. A concrete instance, however wide the class to which it belongs, is necessarily from the very meaning of the term a limited one. It has been suggested, for example, that the coexistence of gravity and inertia is universal throughout all the material world, every particle of matter seeming to be simultaneously and always endowed with both these attributes[1]. This is probably as extensive a regularity of this kind as can be found; for all heavy bodies offering resistance to motion, and heavy bodies existing

[1] Bain's *Inductive Logic*, p. 13.

everywhere, the coexistence is perpetually coming under our notice. So with the coexistence, for which there is much evidence, though we cannot call it positively established, that all psychical activity is accompanied by nervous stimulation or action of some kind. The vast majority of the coexistences to which we appeal for purposes of inference are, of course, of much narrower range than these, and may be found of diminishing range and frequency until we come down to such special and determined coexistences as those of the smell and taste of some scarce fruit, or any of the properties of the very rare minerals.

In speaking here of the 'generality' of such laws of coexistence the word is not used quite in its customary logical sense. A general proposition is properly contrasted with a particular one, and simply implies that the statement is made without exception. 'All bodies are heavy' is not considered more general than 'All English-grown pineapples are pale in colour', simply because generality is not an attribute that admits of degree. But what we are referring to above is not liability to exception, but actual range of prevalence in Nature.

Another small point also deserves notice here. When we speak of 'coexistences', are we to be supposed to mean that of the two or more attributes said to coexist, say *A* and *B*, neither is ever found apart from the other: that is, that both 'all *A* is *B*', and 'all *B* is *A*';—or is it sufficient that a certain one of them is always accompanied by the other, so that only one of the above pair of propositions will hold good? The statement is really ambiguous, and would scarcely be worth pausing over if it did not serve to remind us that exactly the same point is involved here as that which gave rise to the so-called Plurality of Causes in the last chapter. A regularity of sequence, in its common acceptation, is never understood to imply more than that *A* shall always be followed by *B*: we expressly guard ourselves against any supposed implication that *B* must always be preceded by *A*. It was explained that this distinction arose, not out of any difference between these elements in themselves, but out of a difference in our practical attitude towards them. Howsoever arisen, the distinction is easily retained; partly owing to the fact that, the cause preceding the effect, we gain the extra differentiation of *time* between them, partly also to the still-lingering associations of 'efficiency' in the causal

connection. It does not seem unnatural therefore to put the cause and the effect upon a slightly different footing. When however we come to coexistences, the very fact that the two elements concur in time, and the absence of any long traditional association about them, prompt us to be perfectly impartial in our attitude towards the two elements. There is therefore a slight disposition, I think, to interpret a regularity of coexistence as intimating that either of the two elements is a certain indication of the other. This does, of course, hold good in the case of the attributes gravity and inertia, neither of which is ever found unaccompanied by the other.

There seems however no real occasion to interpret so rigidly as this, and it will be best to consider that we have a regularity of sequence whenever any attribute *A* is accompanied by *B*, whether or not *B* is always found to be accompanied by *A*. The really important thing is to be quite clear about the facts. On the general view adopted here there is little fundamental distinction between the two orders of regularity, whatever may be the practical usages or conveniences in respect of their treatment. When we say that *A* is necessarily followed by *B*, but not conversely, we simply mean that we find it desirable to be less precise in our determination of *B* than of *A*. And precisely the same holds good when we make such an assertion of *A* and *B* as coexistences. In each case alike a rigid quantitative assignment of *B* would render the relation of necessary implication a reciprocal one.

As regards the sense in which coexistence is to be interpreted we should have had a good deal more to say here if the question had not already occupied our attention in the first chapter. The reader need only be reminded that it is an expression which has to be interpreted with considerable latitude. When things or attributes are said to coexist, in the logical sense, it does not imply that they stand side by side, or that they must be capable of being grasped in the same act of perception. With many attributes this may be so; but we equally class with them attributes which are locally separated by immense distances, and those which we do not, and possibly cannot, perceive at the same time. A simple example or two will best illustrate this width of interpretation which we are forced to claim.

We say, for example, that the smell and the taste of an

orange always go together. Whenever we perceive one we can confidently anticipate the other. This, of course, is one of those narrower uniformities where the indication is reciprocal. Even from the popular standard of precision of determination neither attribute can be found without the other being found at its side: we never perceive this particular taste or this particular smell except in the fruit in question.

Now simple as such an example is, and near down as we have gone to the ultimate data of sense and consciousness when we are thus dealing with smell and taste, it does not need much consideration to see that even here a considerable amount of mental construction or synthesis has had to be performed before we can regard the smell and taste as permanently coexisting attributes in one and the same object. The acts of perception of smell and of taste are seldom or never strictly coexistent in the sense of beginning and ending together. Like other perceptions they are brief and recurrent, and have to be selected out of a multitude of other nearly simultaneous sensations with which they are not considered to be connected. When this has been done; and the process aided, and the result secured, by resort to language to solidify the groups of elementary sensations, we picture the result to ourselves in the shape of permanent properties or attributes which we consider to coexist in one and the same object.

Consider now the fact that the moon always presents the same face towards the earth. As before, there is what we must rank as a uniformity here; for we know, when we look out at night, what face we shall see. This knowledge, which thus outruns and anticipates experience of the fact, must have an objective basis to rest upon, and such basis can be no other than a uniformity of some kind. Now what is the uniformity here? We are referring to an individual object, and not making any general statement about the behaviour of planets in general. What we must be understood to mean, I think, is this: that wherever the group of attributes by which that object,—the moon,—is recognized and distinguished, such for instance as its size, brightness, distance from the earth, and position, are found to occur, there also, along with these, will be found the characteristic that a certain side of it will always be turned towards the earth. The coexistence is therefore of the same general

description as when we say of the great pyramid that it presents one face to the north. It is a property of an individual. The converse of course does not hold good here. Plenty of other bodies, so far as our statement is concerned, might also persistently turn the same face towards the earth[1]. The uniformity therefore, in the sense in which it is apprehended and expressed, is of a one-sided character: it does not furnish the pair of inferences open to us in the case of the taste and the smell of an orange.

Now when we come to scrutinize the coexistent attributes here,—attributes which our formulæ will commonly represent by the simple symbols A and B,—we easily see to what a great extent the processes of 'filling in' and unifying have had to be carried out, by ourselves and those who went before us. For one thing, the only sense to which we are able to make a direct appeal in this case is that of sight, the object being removed far out of reach of any other organ. And inasmuch as, by supposition, we never see but one face of the moon, it may be asked, Where are the other elements, besides this visible face, which constitute the regularity? Two such, at least, must concur in order to constitute a coexistence; but what else is there, in the way of an object, besides the face familiar to us? The answer of course is that here, as in all other cases, an 'object' or an 'attribute' is a synthesis of a great deal more than our senses can present to us at any given moment. In the case of the heavenly bodies we have an extreme instance of this, for nearly every attribute they possess has to be obtained by such a mental construction. And we cannot do with them as we can with common objects about us, namely help out the sense which fails us at the time by an appeal to the same sense at other times. When I look across the room at a book on the shelf, it is at the moment merely a visible image; but its size and its shape and its weight have been tactually and muscularly felt before, and may be so again. But when we look at the moon, every such attribute has to be acquired directly from other bodies, and then transferred to the one in question.

[1] The characteristic in question is supposed to be the result of tidal friction during past ages. There are reasons for concluding that a similar result has occurred in the case of the planet Mercury, that is, that it always presents the same face to the Sun. (*v.* Sir G. H. Darwin, *The Tides.*)

What, in fact, are the other attributes which we consider to concur with that visible face; or rather with that face specialised by the characteristic of pointing always towards the earth? One of them is an approximately spherical shape. This is founded entirely on reasoning derived from many and remote physical considerations, and certainly not from such sensible data as give me information about the shape of a marble or an egg. We assign it also a certain mass; but this again is the outcome of long and intricate mathematical analysis. Then there is the size and the position in the heavens which it occupies at any given time. But these are given to us merged, so to say, in the visible face which alone we can perceive, and they have to be distinguished from it by an act of mental analysis. It is only after many such processes have been gone through that we can regard the moon as being a persistent unity with a plurality of attributes: that is as being an 'object' in which *A*, *B*, *C*, and so forth can coexist side by side. We obtain the conception of a solid body of a spherical shape, some 2000 miles in diameter, about 240,000 miles distant from us, revolving with a certain speed round the earth, and possessing the characteristic of always turning one side towards the earth.

One caution may be repeated in concluding this part of the subject. The so-called coexistence may often be in great part produced by an unconscious artifice of language by which attributes distinctly and necessarily subsequent in their appearance may be anticipated and made to appear simultaneous. For instance, I may say of a cobra that its bite is deadly; by which I mean nothing more than that the bite is always followed by death. But it is sometimes more convenient to regard the occasional and conditional sequence as a permanent presence of a capacity. This is mainly effected by the aid of language. We term the snake 'deadly,' just as we term it 'supple' or nany-ribbed', thus transferring the occasional sequence to a place among the permanent coexistences. This subject will come before us again on a future occasion.

CHAPTER IV.

THE UNIFORMITY OF NATURE.

In the two preceding chapters we have discussed at length the main characteristics of two very notable kinds of uniformity or order. The former of these has long had its recognized place in logical and metaphysical treatises; the latter, whilst never approaching to the celebrity of its rival, has also had a good deal said for it in recent discussions on Inductive Logic. It is now time to raise the question as to what we mean by Uniformity in general, and whether the two special classes of sequence and coexistence, as hitherto understood, exhaust its range and significance.

This expression, Uniformity of Nature, is used in various senses. By some writers it is narrowed down until it is made equivalent to the Law of Causation, and is used almost interchangeably with that expression. As we propose to use it here it must be understood in a much wider sense, covering in fact every kind of regularity and orderliness. It is very difficult to find any definition for so extremely general a conception as this. Perhaps indeed as near an approach as we can get to any definition is reached by saying that wherever any two or more attributes are repeatedly found to be connected together, closely or remotely, in time or in space, there we have a uniformity. And the general expression, the Uniformity of Nature, is intended to cover all such partial connections, and to imply that their existence, in some form or other, may be detected or reasonably inferred throughout all phenomena whatever.

Uniformity, in this sense, it must be remarked, is the objective counterpart or foundation of inferribility. We are

purposely confining ourselves at present to the objective order of things, because these preliminary chapters are intended to deal with the foundations of inference rather than with the mental act itself. But the two are only different aspects of one and the same state of things. That is, wherever Uniformity exists, there we can, actually or potentially,—in other words with our present resources of observation and calculation, or with such improvements in these as we can conceive to take place without interfering with their essential character,—draw inferences. And where it does not exist, there no conceivable employment of, or improvement in, these faculties would enable us to draw any inferences[1]. What might be done by beings of a higher order than ourselves, we cannot say—their world is not ours—but a system of Logic intended for man must stop short at the ideal limit which we can conceive to be reached by the exercise of faculties like those which we possess. Such a general indication of the Nature of Uniformity as is thus furnished is too vague to be very useful or even readily intelligible: the significance of the conception will best be brought into light by a brief discussion of the principal heads under which it may be divided.

The first two of these are, it need hardly be said, the two familiar classes which have been investigated in the foregoing chapters. We only recall attention to them here in order to assign them their proper place.

1. First then we have the uniformities of sequence. These may for convenience be divided into two subdivisions, according to the degree of accuracy with which we are supposed to be speaking. (We omit the ideal stage, of page 65, here.)

(1) If we interpret the formula with the reasonable stringency demanded for logical and popular scientific purposes, we have what are commonly called Laws of Nature. Understanding these in the general sense in which Brown and Mill may be considered to have popularized them, we may adopt some such statement as this, "If in any two instances the same set of antecedents occur, so will any one of the

[1] Some of Leibnitz's statements of the Law of Sufficient Reason seem to me to approach very nearly to this, regard being had to the fact that they are stated from the subjective side: "Rien n'arrive sans qu'il y ait une raison pourquoi cela est ainsi et non autrement" (Erdmann, p. 748).

immediate consequents,—and, for that matter, the sum-total of immediate consequents."

(2) The above Laws of Nature represent the materials of what may be called first-class popular thought. For ordinary working purposes we require something looser and more convenient. The sequences to which we thus appeal on common occasions are of the kind generally described as *Empirical Laws*, or sometimes in more dignified terms, borrowed from Bacon, *Axiomata media*. They form the staple of our reliance for ordinary purposes.

We sufficiently explained the nature of these empirical laws, as here understood, and will therefore merely add that their characteristic depends upon the lax sense in which the 'cause' is interpreted. Instead of taking some pains to enumerate all the relevant antecedents, we make a selection of a few of the most important. We thus obtain a sequence of far wider prevalence, but one upon which proportionally less reliance can be placed.

II. In the next place there are the Laws of Coexistence. These have received far less notice than those of the former class, and have been generally disparaged in comparison. It was explained that the two classes were strictly analogous in all essential respects, and that they would admit therefore of the same subdivision and arrangement, viz.:—

(1) Moderately strict coexistences, in which reasonable care had been taken to include all the elements. In such cases, —due regard being had to all the difficulties involved in the attempt to distinguish between the attributes,—there seemed no reason to deny to coexistences any of the rights of inference to which the sequences are considered to be entitled.

(2) Loose coexistences, in which but few elements were introduced, possibly only two; one of these being taken as a sign of the presence of the other. The dependence to be placed upon these is, of course, not very strong; but such as they are, they form the foundation of our conclusions on a vast number of occasions in our ordinary life. The question to what extent, if any, these coexistences admit of resolution into sequences, is reserved for discussion in a future chapter. Before proceeding to the next classes of Uniformities some extremely important considerations must be attended to in

reference to the above two. It is not so much that the assumption we are now about to make is one which would in common language be regarded as a 'uniformity', but it certainly seems demanded in order to render the others available.

A convenient way to introduce this assumption is by the following query. Conceive that some ingenious and malicious agent were endowed with complete power over all the properties and forces of Nature, to make and to mar at his will, and that the general problem were set before him to effect such disturbances as should entirely put a stop to all inference, and therefore to all safe and rational action, on the part of man:—what had he best do? We will suppose that he is recommended to do his work efficiently but economically: that is, he is to make the minimum of change which will answer the proposed purpose. What then would be the sort and amount of mischief he would find to do in order to destroy at a stroke all the fabric of Inductive inference whether of daily or of scientific life?

Were such a question proposed to logicians of the school of Brown and Stewart, it is likely that what they would reply would in effect be this:—Just let all the causal 'chains' be snapped or corroded, so as to be no longer trustworthy. (We may remark here, in passing, that it would come to the same thing whether the objective regularity itself were tampered with, or merely our belief in it, or whether both were involved in one common ruin. What is necessary for successful action is the concurrence of both: the absence of either would be fatal. This however will be discussed in the next chapter; what here concerns us is the objective regularity, with which alone our agent is supposed to be allowed to meddle.) They would say that if the universal prevalence of the Law of Cause and Effect were interfered with, the whole fabric of successful thought and action would be pulverized at once. And so it would, no doubt. If the same antecedents could no longer be insured to give the same consequents, this loss of confidence would produce a shock which would paralyze all thought and action. There would however be a quite needless violence in setting to work in this style: much less extreme measures would quickly bring down upon our heads a state of things not a whit less

mischievous for any practical purposes. If I had the work entrusted to me I would, like any prudent reformer, express unbounded respect for the present constitution of things. I would not touch the Law of Causation; in fact I would interpret it with the utmost stringency and rigour. Let it remain true hereafter, as before, that the same antecedents shall be followed by the same consequents:—only neutralize the efficiency of the formula by securing that the same antecedents do *not* recur. A great deal more than is commonly supposed might be quietly effected in this way; without even doing anything which would fall under common definitions of a miracle. For instance, let each pair of animals, plants, fruits, and so forth, be unique of its kind, like the fabled phœnix,—we might add to the number of species in proportion as we diminished the number of their representatives, so as to keep up the quantity of individuals and add to the consequent perplexity,—and most of the generalizations and inductive extensions upon which we depend for guidance in daily life would vanish at once. Again, when we came to deal with material objects, a sudden and sufficient increase in the motion of the earth,—always leaving the law of gravitation absolutely intact,—would destroy a great proportion of the regularities on which our conduct proverbially depends: the seed-time and harvest, the day and night, and so on. For if the earth were sent out into a hyperbolic path we should never again have any one summer or winter or day or night which would be an exact repetition of the preceding one: nor would even an average of any number afford safe guidance as to the repetition of a similar average again: and such a change of course would carry its consequences throughout the whole of animate Nature.

The above are but one or two instances out of multitudes which might be suggested. It is obvious what an amount of mischief our agent, if he properly understood his business, might effect without tampering with laws of causation, or at most without doing so at more than one point and once for all.

What this fanciful hypothesis is intended to illustrate is the insufficiency of the Causation formula, when rigidly interpreted in concrete sequences, to serve by itself as a means of inference. It *can* be thus rigidly stated, as we have shown, without

difficulty or inconsistency, but it has then to be couched in a hypothetical form. To render any definition of this kind of the slightest use we must append a rider to it, by the assumption that such repetition of similar cases as we require does actually occur. Otherwise we could never apply our formula, for the most infallible rule will fail us if we never encounter the circumstances in which alone it is available.

Repetition therefore of similar cases is essential, if we are to utilize the uniformities, but the way in which these repetitions are brought about deserves careful notice. Complete repetition of all the constituent elements, down to the most minute, is, as we have already seen, out of the question. Such repetitions as we actually find set before us are the result of two factors, one contributed by Nature, the other partly contributed by ourselves. There is, that is to say, an actual recurrence over and over again of a large proportion of the elements which compose the antecedent, but with this must be combined the knowledge on our part that the elements which we decide to omit, in order to secure the recurrence, are really insignificant. This involves a considerable amount of acquired information as to what is trivial and what is important, in each class of cases. There was a time when any one who wished to decide what would be the result of a battle or an expedition would have thought that amongst the essential antecedents was to be included the position of the stars at the time, or the fact of a bird being seen to the right or left of the observer. We have decided that such incidents are unimportant, and accordingly we can recognize recurrences of similar instances in cases where our remote ancestors would have thought the circumstances widely different.

Nature, as we have seen, and as Leibnitz was fond of insisting, never exactly repeats herself. But she does the next best thing to this. She gives us repetitions,—sometimes very frequent, sometimes very scarce, according to the nature of the phenomena,—of all the important elements, only leaving it to us to decide what these important elements are.

One way indeed in which these repetitions are presented to us is in the form of what we call coexistences, which brings us round to the point already discussed. In fact a coexistence loosely understood *is* an occasion of a repetition of a sequence similarly understood; that is, it is an occasion of recognizing

and applying it. This may need a moment's consideration. Recur to the lobster which we found to turn from black to red on being put into boiling water. Here is a sequence which we feel confident will be repeated *if* we repeat the antecedent. Now if there were but one lobster available this certainty, being merely hypothetical, would not serve our purpose. At this point come in the coexistences to help us. The lobster, being a 'natural kind', belongs to a class containing a plurality of members, and all of these furnish us with the repetitions of the previous sequence if we choose to avail ourselves of them. The lowest or narrowest such uniformity of coexistence which would fall under our description of them, must at least contain two members, and even thus we should secure one repetition and so could put our sequence to proof. But since in practice there are mostly many specimens of each kind, the coexistences become the main opportunity afforded to us of converting our sequences from hypothesis to actuality.

A few remarks may be added here as to the licence open to us, in various cases, to omit determining or individualizing circumstances. It is only by such omission, as already remarked, that we can ever secure repetitions of what we regard as the same phenomenon or event. A simple example or two will serve to bring out the distinction.

For instance, I take pieces of copper and of zinc, connect them in a certain way, and plunge them in an acid: a galvanic current follows. Now we all admit that if exactly the same process be repeated the same consequence will follow. But can the same process be repeated? Obviously it can up to a certain standard of precision. As we are dealing with voluntary actions we have not here to sit down and wait for another occasion of the same kind to present itself: we can ourselves make, or rather aid in making, our repetition. We can select other pieces of metal, and more acid, of the same quality, and treat them as we did before. We are thus appealing even here to the repetitions which Nature so freely affords us in the case of simple substances, and by their aid we secure the desired opportunity of testing or applying our sequence.

We cannot indeed do exactly the same thing over again, as we should soon find if some exceedingly delicate operation had to be performed which turned on the precise quality and

strength of the electric current. But it is soon found that for practical purposes many circumstances may be omitted as trivial,—the temperature of the room, the moisture of the air and so forth. Our power of thus recognizing insignificant attributes, and the fact that we are dealing with voluntary actions about comparatively simple substances, secure us as many repetitions as we require.

Now take the following case. I drink a quantity of sherry one night, and wake with a headache next morning. As before, I feel no doubt that if exactly the same thing were done over again exactly the same result would follow. Here too, as before, there are a number of notoriously insignificant circumstances, such as the moisture of the air, its electric condition, the direction of the wind, and so forth. And again, as before, there is a group of circumstances which we can repeat with tolerable accuracy, such as the strength of the liquor, its quantity and quality, and the time of its consumption. So far no difference of importance can be detected. But whereas in the former instance these important attributes admitted of exhaustive determination, and tolerably accurate reproduction, in this latter instance many of them,—those, for example, which are included in what is called our state of health at the time,—do not admit of determination. We cannot therefore procure two cases sufficiently resembling each other to give the Law of Causation fair scope to show what help it can give us. The available cases are sufficiently in agreement to raise a presumption, but not sufficiently so to produce confidence.

One more example. I take a box of dominoes, toss the contents to the ceiling and mark the faces and directions of the pieces as they fall. As before, none of us has any doubt that repetition of the antecedent will be followed by certain repetition of the consequent. As before, some of the antecedents can be repeated, for we may throw the same pieces in the same room; and some, as before, are notoriously ineffective, such as the hour of the day and the phase of the moon. What baffles us is the vast number and impossibility of determination of really effective elements. Few conditions in fact can be so remote that we can make sure that they have no influence: even our state of health, and the temperature of the room, have something to do with the result. Consequently we cannot obtain any-

thing that will pass muster as a concrete repetition of the event in question. That event is, to all relevant intents and purposes, in the position of a unique one. The repetition is wanting here which alone could render the uniformity of sequence available for us.

III. The next class of uniformities which deserves notice here may be briefly described as being of a *rhythmic* character. As we are merely passing them in review here we will not stop to enquire whether they may not by a stretch be brought under the head of sequences and coexistences; but for practical purposes they are best put in a class by themselves.

What is referred to here are those broad cycles of recurring events which may be traced in almost every direction in Nature. They deserve separate notice if only for the fact that their immemorial recognition, and their enormous importance, have gained for them a quite proverbial acceptance as the type of natural stability giving ground for human reliance.

The cycle of recurring events constituting day and night, and the similar cycle constituting summer and winter, are as above suggested the most prominent and familiar of such instances. In several respects such groups as these correspond to 'species' of things, or the other 'natural kinds' described by Mill. That is, they furnish us with a large number of similar groups, agreeing in the bulk of their important elements, but differentiated by a number of comparatively insignificant details. They therefore furnish us with numerous and convenient opportunities of applying our sequences, and thus setting the Law of Causation to do our work. I sow seed, and it flowers and ripens that year. In order to do the same thing over again, and thus to be able to anticipate the same result, I must obtain a repetition of such a cycle. And thus a group of 'seasons' comes, for these logical purposes, to resemble the kinds of substances or living organisms which as we saw gave us many of our opportunities of similar repetitions. It would not be correct to speak of these cycles as 'coexistences', for the bulk of the elements which constitute them are distinctly not simultaneous but successive; but inasmuch as this group of elements does not display the causal characteristic of rigid regularity distinctive of strict laws of sequence, they are best classed with the coexistences. An example will make this plain.

The light of day, increasing from dawn to noon and then declining to twilight, gives us a succession of events one of which does not 'cause' the other, even in the merely sequence sense of that term; the group is therefore more in the nature of a simple aggregate such as constitutes the popular notion of a Kind, that is, an uncaused group of attributes. We cannot call them strictly a coexistence; but then, as we have already seen, there is a great deal of conventional assignment and interpretation involved in every case in which we speak of a coexistence.

The rhythmic character of natural phenomena illustrated by these cycles has received much notice from Herbert Spencer. He regards it as a necessary and universal characteristic of Nature, and as one which admits of a sort of à priori proof. That these cycles are very widely spread is certain; but so far from regarding their existence as necessary it seems to me easy enough to conceive an alteration which should at once mar their character and eventually destroy them. Just such a change as we recently supposed our mischievous agent to carry out for the purpose of baffling our predictions would suffice to do the business. The known Laws of Motion in no way demand an elliptic orbit in a planet; they will be equally satisfied with a hyperbolic orbit. And then the rhythm of summer and winter begins to suffer, and slowly to tend towards the dull monotony of one unchanging temperature. So with the day and night. Let space be filled with a resisting medium (and we certainly do not know that it is not) and the familiar rhythm that we now experience would gradually begin to be affected, and finally to disappear.

It does not appear correct, therefore, to regard these cyclic arrangements of phenomena as in any sense necessary or ultimate. They have their conditions,—in the case of the seasons a certain velocity of translation of the earth is demanded, and if this be exceeded, the cycle no longer remains unimpaired,—and so long as these conditions prevail that characteristic will be found, but no longer. This does not however diminish their practical utility, and they therefore deserve distinct notice in any discussion of the foundations of Inductive Logic.

IV. The Conservation of Energy. This now familiar

physical principle certainly deserves a place amongst the uniformities which furnish a ground for inference, though in a logical treatise our discussion of it must be very brief. The main characteristics of it which claim notice here seem to me to be the three following, in all of which it represents an immense advance beyond the mere regularities of sequence which play so large a part in our older logical treatises.

(1) In the first place it embraces a whole field of inference which sequences cannot reach, or can reach only with a certain strain. Suppose for instance a ball rolling along the ground, which gradually slackens in speed and at last comes to a stop. We can calculate its speed throughout, and therefore make inferences about it; but we should find it hard to apply any reasonable modification of the common account of causation to such a case. Of course if it were a *repetition* of a precisely similar performance that account might be introduced; but it is surely a rather futile interpretation to say that, *if* another ball were set rolling just as that one is rolling, it would follow the same course throughout.

Now what the doctrine of Energy does here is to supply a principle which requires no appeal to any other similar concrete example. The energy of motion with which the ball started must be retained in some form: what the ball loses in motion the ground and air must gain, through friction, in warmth. We must appeal to experience to ascertain the friction, and we may be unable to work out the problem accurately, but we have the requisite data in our hands.

The fact is that the ordinary sequence formula, as given in the Brown-Mill Law of Causation, is only appropriate where we are dealing with concrete cases of distinct, almost abrupt, change. In the words of Mill himself, "it is events, that is to say changes, not substances, that are subject to the Law of Causation" (*Exam. of Ham.* p. 295). And the great advance indicated, in this respect, by the doctrine of Conservation of Energy is its perfect appropriateness to cases where there is an entire absence of change (where the Energy is preserved unchanged); as also to those slow and continuous changes, as in the gradually stopping ball, where the energy is very slowly modified into other forms.

(2) In the second place the doctrine bridges over the

chasm between different classes of sequences which the old formula had to leave unconnected. We knew, for instance, that a certain chemical action would produce electricity, and we knew that friction would produce heat; but between heat and electricity we might have had no link. No doubt we could in this case also have appealed to specific experience, by securing a concrete instance of the right kind, but the advantage of the principle in question is that it enables us to dispense with this, and therefore greatly widens what may be called the actual as against the simple hypothetical scope of our formulæ. We can say with confidence that energy cannot be lost: that it must exist somewhere and somehow, in one or other of a certain number of forms, such as motion, heat, electricity. And if we are able to state in any given case which of these forms must be assumed, then we can at once link one of these causation-connections on to another, and bring into play sequences which would otherwise be unavailable.

(3) The indestructibility of Energy, under so many and strikingly different forms, is doubtless its most impressive aspect; and owing to the light which this characteristic sheds, and seems still more capable of shedding, upon the processes of Nature, it has deservedly been the most discussed in popular explanations. But for purely inferential purposes it is a question whether the importance of the principle is not most felt in a third respect, viz. the enlarged scope it yields for *quantitative* assignment. The indestructibility of *mass* is the foundation of much of the accurate work which physicists and chemists have attained to in the past; and it seems probable that the admission of a similar indestructibility of a non-material element, like this of Energy, may do as much for us in fresh directions in the future.

As we are expressly confining ourselves, in this early part of the subject, to non-quantitative considerations, only a few words of indication can be given here. It must suffice to say that the knowledge that no portion of Energy can be lost is an enormous help in our power of inference. One of the forms having its exact equivalent amongst the others, in respect of quantity, we can select the most delicate and convenient of these forms for the purpose of measurement. And when we know and are able to measure approximately some of the forms which are most

sensible, we may succeed in indirectly estimating others whose direct measurement would be quite beyond our powers. In these, and many other ways, some of which will receive notice hereafter, the principle of Conservation of Energy has become one of the most powerful resources of the physicist wherever accuracy of measurement is involved.

V. The next class to be noticed contains those which, for want of any well-recognized descriptive term, may be called *Probability* uniformities. Owing to the special mathematical treatment demanded in this class, we must notice them but very briefly here. They form the basis of the Theory of Probabilities, being the objective counterpart of that graduation of quantity of belief which the Theory assumes in its subjective treatment.

Their main characteristic may be indicated in a few words. They combine individual irregularity with aggregate or average regularity; they represent therefore a distinct advance of the domain of uniformity into regions which the preceding classes did not attempt to grapple with. In each of those former classes we were as certain of the individual case as we were of the group to which it belonged. We felt exactly the same certainty that any particular *A* would be followed by *B*, for instance, as that *A* in a definite average proportion of cases would be so followed. And if we felt quite uncertain about it in any given individual case we did not look for any better information in dealing with a number of cases. For instance, in the galvanic battery, we felt just as certain that the current would be produced in the next case we tried as that it would in a certain proportion of the cases which formed the generalization of our inference. And in dealing with the box of dominoes we felt exactly as uncertain about the one throw as we did about the result of a number of similar throws. At least nothing was there intimated to suggest a difference between the two cases.

It is now found however that a broad distinction must be drawn between individual cases and averages of a number of individuals, and that it is quite possible for us to be in absolute ignorance as to the occurrence of an attribute in the former and yet to be quite confident about its appearance in the latter. When a die is tossed fairly no resources known to man will put one person in a shade better position than any other person in

respect of inference about the next cast. No reasonable being raises a hint of suspicion that the Law of Causation does not in reality apply to that case, but the fatal defect already alluded to,—namely the impossibility of securing an adequate repetition of the antecedent,—prevents us from appealing to any such law. But when we turn to averages the case is widely different. If it be asked, for instance, whether in throwing a pair of dice we shall in the long run get an average of at least six points, the instructed person is at a distinct advantage. He can not only say that we shall do so, but he can say with numerical accuracy what proportion the failures and successes will bear to each other on the average.

The special conception prevalent throughout this Calculus is the substitution of a mean or average for the individual case; and the physical foundation which justifies us in doing this is the constancy or orderliness of the mean in certain fields of enquiry. This constancy is by no means universally prevalent, but where it does prevail it opens out to us immediately, like any other uniformity, a fresh province of Inference.

For reasons which need not be discussed here the practical treatment of Probability soon leads us into mathematical calculation, and into a sort of calculation which is apt to become extremely intricate. But the general conception which is involved in it is by no means a difficult one to grasp, being merely that of regularity which gradually displays itself as the area of observation is extended, and which is perfect only as a "limit" in the mathematical sense of the term.

VI. The last of the classes which it will be convenient here to indicate consists of what we may call 'Uniformities of Persistence'. We may intimate their nature by the following example. Our plain man cuts down a tree one day in the forest. He goes out again the next day to set to work on the next tree of the same kind. Every logician will tell him that he will have to trust to the Law of Causation or Sequence, by relying on the same consequent following the same antecedents. Most logicians also will remind him that an appeal to Laws of Coexistence is involved, for he will naturally assume that the grain and texture of the second tree will resemble those of the first; and this, according to Mill, rests on the assumption of a group of uncaused properties repeating itself in a natural kind. So far

the ground is familiar; but our woodcutter will also instinctively and with equal confidence expect to find the trunk of the first tree lying where he left it, and in the same general condition.

Now on what principle is he depending? That there is an inference here, in the sense of an anticipation before specific experience, exactly as truly as in the case of the other two conclusions, admits of no doubt. And it also seems to me clear that we cannot force such inference to fall under the general statement of the Law of Causation, without some violence. In fact, on Mill's definition, it would be inadmissible to do this, since he expressly lays it down (as quoted above) that Causation only applies to changes, and the one characteristic of the state of things in question is that there is no appreciable change about it.

If we were to succeed in eliciting from the man himself what were his grounds of confidence in each case, we should probably have them in some such forms as these:—that of course if you cut the second tree as you did the first, it will fall like the one before it: and that everybody knows that the same kind of tree shows the same sort of grain. So far the logician would go along with him, though he would think that the language admitted of amendment in respect of precision. But in the third place the man would pretty certainly deliver himself to the effect that of course he expects the tree to remain where he left it, until he or some one else removes it.

To this account it is quite possible that the logician may object that to speak thus is idle, for it is only to say that a thing will not move unless it be moved. If he did take this ground it would, surely, be shortsighted on his part. For one thing, his plain friend might retort that other logicians as acute as himself had brought precisely the same objection against his own Law of Causation:—Mansel we know scoffs at Mill's definition of this Law, declaring in so many words that it is merely the old nursery rhyme over again, "if she's not gone she sits there still." (Proleg. Log. p. 331.) I do not think that Mansel is quite right here, for we have seen that with due interpretation and assumption the Law might be made significant, and really helpful to us. But nevertheless the objection is an awkward one for a logician to raise.

If we ask the men of physical science what we are to make of such a generalisation as is needed here; they would probably say that the confident conviction of the plain man, that things will remain for some time in pretty much the same state as that in which they were left, stands in a similar relation to the conviction which *they* hold, that the crude popular view of causal succession does to the scientific refinements we have already considered. They would substitute the comparatively abstract and perfectly precise statement that 'a body will remain in the same state of rest or motion unless it is acted on by some external force'. This deals with the log of wood as a whole, and of course leaves it to us to decide whether or not any external force has been able to act upon it or not. But this statement (known as the first Law of Motion) is absolutely true, so far as we can test it, and is so far from being a mere matter of definition that it forms a portion of the basis upon which the whole fabric of scientific Astronomy is built.

As regards the practical conviction that not only will the log as a whole retain the same position but that it will not have fallen into pieces or rotted to powder, nothing corresponding to this precision can be offered. The nearest approach to anything of this kind,—we are speaking of course of laws of real generality and not of any which would appeal to chemical or botanical considerations,—would be what is sometimes called the Principle of Continuity: in other words the old *dictum*, 'Natura non facit saltum'. In any accurate sense, what we formerly said about the last refinements of the causal relation holds here, namely that all that such a principle can yield is an initial tendency: it cannot enable us to step with an inference over any discrete interval however small. All that it denies is actual discontinuity; that is, instantaneous finite change. It assures us that if at any period at which a change,—however irregular a change,—is proceeding, we diminish without limit the interval of time under consideration we shall find the change, during that decreasing interval, tending to grow more and more uniform [1].

[1] Leibnitz's version of this doctrine is worth quoting:—"Lorsque la différence de deux cas peut être diminuée au dessous de toute grandeur donnée *in datis* ou dans ce qui est posé, il faut qu'elle se puisse trouver aussi diminuée au dessous de toute grandeur donnée in *quæsitis* ou dans ce qui en resulte" (*Sur un Principe générale, utile à l'explication des loix de la nature*:—Erdmann, p. 105).

Our practical confidence that as we left a thing one night so, or somewhat so, we shall find it next day, rests mainly upon a precarious but generally justifiable extension of this principle by analogy. We gradually come to learn the main classes of exceptions to it, and to feel some degree of confidence that, these apart, what is rigidly true in the limit will be sufficiently near the truth for practical purposes at some considerable distance from the limit.

The reader may think that it is unworthy of science to take notice of such considerations as this. But if we propose to systematize the principles on which we do actually depend when we are drawing our daily inferences about future or distant matters of concrete fact, surely no doubts can be raised as to the legitimacy of their introduction. The metaphysicians have been too much in the habit of treating the Law of Causation as if it was not only expressible in a rigidly accurate form, but also capable of practical appeal in that form, and this has set up an altogether false standard of certainty to the inductive logicians, who have naturally wished not to fall behindhand in respect of what they claim. Accordingly there has been too much of a tendency to omit, as inadequate, whole classes of considerations upon which many sound and reasonable persons unhesitatingly rely every day of their lives in speculation and practice alike.

A number more of generalizations, or wide uniformities, might easily be added to the above; but the attempt to enumerate them would soon lead us into the province of this or that special science, which is generally far more competent to treat them. For instance, the Laws of Motion, as expounded by Newton and his successors, are as wide in their range as the material world. In the domain of Mechanics, and therefore of Physical Astronomy, they call for explanation and justification; but they fall somewhat short of a claim to treatment in Inductive Logic.

There is indeed one wide uniformity which ought not to be passed by entirely without notice, since it has received the support of Herbert Spencer. I allude to that tendency towards Differentiation, which he regards as of universal prevalence and as admitting of à priori proof, and which he terms "the Instability of the Homogeneous." That this principle holds

true in a great variety of cases is indisputable, as no one has better shown than its author; but it seems to me to be of a highly derivative character, and to depend upon so many conditions that the exponents of each separate science must determine to what extent it can be admitted within their borders. Surely it is pushing its claim too far to regard it as necessarily, or indeed as universally true. That the process of continued differentiation has prevailed over vast periods of time cannot be doubted, but most physicists contemplate a time when the reverse process may set in. In what stage is the moon now? As far as we know, nearly all differentiation has long since come to a stop there, and it is generally held that a similar fate distantly awaits every other planet, and that at some remote epoch they will revolve round an extinct sun in everlasting night and cold.

Plenty of further instances might be alleged which, if unimportant in themselves, seem quite capable of setting aside the claimed universality of Mr Spencer's Law. The physical constitution of the Ocean apparently tends on the whole towards uniformity, and is more than able to suppress the perpetual efforts, so to say, towards differentiation caused by the diverse contributions brought in by various rivers and the erosion of various coasts. So with the atmosphere. The local disturbance caused by some vast volcanic eruption does not become the starting point of continual further disturbance, but is steadily overcome and at last disappears altogether. Here as in not a few other cases, the tendency is towards the homogeneous.

CHAPTER V.

THE SUBJECTIVE FOUNDATIONS OF INDUCTION.

We have now examined, with sufficient minuteness and care for our present purposes, what may be called the objective or material foundations of an Inductive system of Logic. But, in accordance with the general view already insisted on, such an examination deals only with one side of our subject. Logic is neither a purely objective nor a purely subjective science, but essentially and almost exclusively a science which involves both aspects. It concerns itself with the operations of the human mind when drawing inferences about the phenomena of nature. Accordingly we must now enter into some examination of the second, or mental, side of the enquiry, by ascertaining the nature of the postulates which have to be demanded from the regions of Psychology or Metaphysics before a System of Inference can be constructed.

I. In the first place; it is obvious that the ordinary powers of observation must be taken for granted. Logic, by universal admission, in every application we make of it, starts from premises which have been obtained from observation, directly or remotely. We must therefore include, amongst our postulates, the existence of these powers of observation. As however this is in no way peculiar to Logic, but applies in an equal or even greater degree to many of the special sciences, we need not pause to examine it as a general postulate.

Where the question does force itself upon our notice,—and indeed, as we are about to see, raises some very perplexing problems,—is not so much in respect of the mere assumption of these powers, or in the assignment of their general character, but rather in the attempted determination of their boundary line. Where, in fact, are we to suppose that pure Observation

ends and true Inference begins? In a Science of Inference such a question as this is a serious one; and it must be frankly admitted that any doubts and difficulties which we encounter in answering it constitute a flaw in the theoretic perfection of the science. Unfortunately however there seems no way of completely removing such doubts, and all that we can do is to minimize their consequences.

Any simple example will serve to illustrate the difficulty. Suppose I am on a walking tour, and a stranger proposes to join our party; I give a glance at him and say to my friend, 'I can see plainly enough that *he* will not be fit for our excursion to-day'. Now though this remark is couched in the language of mere observation any one uttering it would not need to be reminded that it is a mixture of observation and inference; and if he spoke with less colloquial abbreviation he would intimate the distinction by expressing himself somewhat as follows,—'I can see that the man is ill, and therefore I am sure he cannot take a long walk'. In common parlance the present illness is an observation, and the inability to take the walk is an inference. We might not be consciously thinking of the distinction at the time, but this is the sort of analysis we should instantaneously make when attention was directed to the point. Our plain man would reply, 'you can see for yourself the state he is in. Just look at him, how ill he is', and so forth.

Now it is a merely elementary step in analysis to point out that the assumed state of the man, bodily and mental, which is involved in the 'illness', is largely a conclusion founded on data. The very expression 'symptom', so commonly applied to diseases, is an illustration that the distinction has been recognized as far as this by all but the rudest and most unobservant. So far then we have pushed the observation a stage further back, having resolved it into such elements as the paleness, the lax or stooping gait, perhaps the quickness of breathing, and so forth, which are considered to be the symptoms of the disease.

But then begins again the never-ending process of analysis as applied to these elements themselves. For shortness, take but one of these, the paleness, where we are purposely confining ourselves to a characteristic which seems about as simple and elementary as experience can furnish: it is one of colour pure and simple. But the psychologist has something to say about

this. It admits of simple proof that the colour of the man's face, as perceived by us, varies vastly more according as we see it by daylight or candlelight, or even according as he stands somewhat more or less in the shade, than it can possibly vary according to the extremest conditions of health and sickness whilst the light remains the same. That is, our subjective estimate of such a simple and apparently ultimate datum as this of mere colour is in great part an instinctive judgment or inference. What we really saw is so instantly corrected and allowed for that it actually drops out of notice, whilst what is effectively retained is something so different from the former that it must be regarded as very largely consisting of inference.

Again; suppose that by an effort of reflection, and comparison of the same shade under varying conditions, we had enabled ourselves to estimate the colour as it was, that is, as it should be under normal circumstances,—and the psychologist knows how difficult this would be,—was it really true that we saw, as we supposed, a *surface* of that colour? It is highly unlikely that we did so. What any ordinary glance takes in, when directed towards a surface, is nothing more than a succession of points which are supplemented and filled in by something else than sight. At least this is all that is perceived by the central spot of the retina, which alone is capable of clear vision. How obstinately our senses refuse to undertake the drudgery of examining every separate detail in the objects we inspect, even when we are gazing upon them with some care, is only too well known to those who have ever worked through a proof sheet as it came from the press. The almost inevitable impulse is to visualize a few letters and thence to infer the whole word, and even from a part of a sentence to infer the rest; and it requires a strong and persistent effort to insist that the eye shall not thus shirk its work of adequate observation.

Finally; take as minute a fragment of visible area as we choose, so as to avoid any such spatial filling in as that just indicated: is the impression really continuous, either in time or space? Confine ourselves, for the sake of brevity, to the former continuity. It is approximately certain in the case of sight, and quite certain in the case of sound, that what seems to us to be

a continuous elementary impression is really made up of distinct nervous impulses or shocks. We are not referring here to the fact, familiarly illustrated by the case of a rapidly revolving point of light, that *finite* impressions outlast their producing cause, and so tend, when repeated after short intervals, to overlap and become continuous. We are here going a stage further back, and are enquiring into the mode of production of the most elementary and briefest of such finite impressions themselves; that is, we are referring to the process by which impulses or shocks which separately do not emerge into consciousness can yet do so when there is a sufficient succession of them. The fact itself must of course be taken for granted here; the only question now before us being whether the distinction between datum and inference which has been pushed thus far back, is to be considered capable of receding one stage further still. There are many psychologists who distinctly claim these non-conscious elements as being as truly 'mental' as those of which we are conscious; are we then to admit that the step from the one to the other is to be regarded as a logical step, and as being of the nature of inference?

As we have not yet come to examine into the real nature of inference in the cases in which its existence is undisputed, it would be impossible to attempt to decide this question properly here. We will merely indicate in a few words why such a step as this last is not to be ranked as a logical one. Briefly, then, Logic is concerned, not necessarily with processes of which we are conscious at the time,—for many unquestionable inferences take place spontaneously, and without our being aware at the time that they are such,—but at any rate with those that can be voluntarily reproduced when attention is directed to them. This seems the most definite and convenient point at which to mark the line. In all the successive cases indicated in the foregoing description, except the last, the process seems essentially to be of the same character. We had mentally taken a definite step from one conscious element to another: often no doubt without knowing that we had done so: but it was always a step which we could, if we pleased, go over again deliberately. We felt that we could revise or justify our judgment. But the step which leads us *into* consciousness is a very different one from that which only leads us from one point

to another within its province. The data here were such that no amount whatever of introspection could possibly set them before us directly: we can only reach them indirectly by analogy, not start from them deliberately.

The general conclusion I should draw in respect of our attitude towards any really ultimate data, is that they can no more be reached than can a first point or absolute limit in time or space. Everywhere, however far back we may succeed in pushing our analysis, we find ourselves in the same general position:—that of having something in hand which implies something beyond or behind it. The metaphor of the ever-receding horizon which we follow in vain if we seek to find a terminus there, seems a sound one. We cannot start from the horizon and work our way steadily from this as a beginning, up to the point at which we now stand: our path is in the opposite direction, ever straining towards something which it is impossible for us actually to attain.

The popular estimate of the claims of Logic is, presumably, that it *has* a definite starting point: that if we do not attain ultimate data it is merely because we have not taken the trouble to go back to them: that sense or intuition can always furnish them for us. This view is supported by a stock of common metaphors, which, whether they conceive our path to be an upward or a downward one, whether, that is, they liken it to a chain hanging down, or to a building rising up, always suggest a definite starting point. The links must have some fixed attachment, whence they hang firmly: the courses of masonry must have a solid foundation, on which they rest securely; and so forth. All these metaphors are misleading, unless it be expressly explained that any such starting point is a merely conventional one, assumed for convenience. Everywhere, wherever we may happen to find ourselves, we are in possession of data which are familiar to us and are justified by experience. These are our starting point, and not any really primitive data. From thence we proceed, so to say, outwards, always striving towards absolute origins or elementary data, but without the slightest hope of ever reaching them.

The attitude of most ordinary persons towards the distinction between observation and inference is quite in harmony with this view. They do not indeed deliberately recognize that no

ultimate elements can ever be obtained; they do not much trouble themselves about any such consideration. What they primarily have in view is not the distinction between observation and inference, but rather that between what may conveniently be taken for granted and what needs reasons for its support. The two distinctions are not quite the same thing, but they run nearly parallel. When, in our example some pages back, the speaker says that he can see that the stranger is not fit for the expedition, all that he has in view is that such an opinion will be readily accepted. On this being questioned he falls back on the statement that he can see the man is ill, claiming that *this* at least will pass without question. And so on, step by step. He is not thinking of anything so technical as 'pure observation' and where exactly this may be detected, he is only thinking of what will be admitted then and there by those to whom he is speaking; and he is prepared to go as far back, step by step, as may reasonably be expected until he and they come to some common basis of agreement. But he would naturally soon become irritated with any one who kept up the analytical cross-examination too long, on the ground that it was quibbling about points which no rational person could doubt.

II. Again; we must clearly postulate the faculty of *Memory*. As this also is an assumption equally demanded in every science, and in all the operations of life, only a few remarks need be offered to indicate the directions in which the appeal becomes specially prominent in Logic.

That memory is necessarily involved in every act of reasoning, that is, in every passage from premises to conclusion, is clear. It is as much required as is molecular cohesion in Physics to yield the solidity of matter. A mind that could not keep hold of two propositions during the time it was putting them together in order to infer a third, could not rationally reach that third proposition at all. Similarly with the collection and retention of the details which are summed up and expanded in an inductive generalization. Still more in the practical reasoning of common life is it necessary that the mind should be already well stocked with an abundance of propositions which may serve for premises when they are called for.

In such cases as the above, memory is too obviously appealed

to for it to be possible to overlook the demand; but there are other directions than actual inference in which a like appeal is essential, and as some of these are not likely to attract the notice of those who have paid no attention to Psychology, they may be briefly indicated here.

Take, for instance, a simple proposition; that is, one which involves only two terms and a copula. To ordinary apprehension they appear to be bound up into an almost indissoluble unity, as if they were actually simultaneously grasped. There is something like this even in the more complicated process of reasoning. When the logician draws the process out into regular syllogistic form, he is often as it were employing a telescope to resolve what was taken for a single point into three distinct stars. Much more so in the case of a simple proposition, where the resolution into discrete elements requires a more conscious effort. Memory, however, in the sense of retention, must have been employed. The terms succeed each other at appreciable intervals; and as the organic process of enunciation thus takes a perceptible time there can be little doubt that the mental synthesis accompanying this process is also carried on at a finite rate. But who is conscious of this at the time? A good illustration of the rapid and unconscious nature of this process is afforded by the apparently capricious way in which different languages arrange the subject and predicate. This is so, even in the same language, according as we are speaking poetry or prose. When for instance we say, 'The horse was black', or 'black was the horse', we feel a certain difference no doubt; but this is probably no more than what arises from the vague emotional associations connected with the poetic form of construction. But who recognizes any difference in the actual process of framing the sentence, or rather of framing the resultant mental concepts corresponding to it? And yet if each step of such a process were consciously realized it would be no parody to say that in the one case we think of a black surface and then proceed to trace out a horse upon it, whereas in the other we start with the outline of a horse and then proceed to paint it black. The proposition as consciously entertained forms a single element which we do not pause to analyse into its parts. A brief act of memory has fused them into one as completely as two stars close together are seen

as a unity; but memory has been at work to carry this out.

It is not for a moment implied that this sort of fusion takes place in every proposition. There are, in fact, numerous exceptions. In the case of negative propositions, as in those which are regarded as doubtful, the two constitutive elements are generally kept more or less apart in the mind, and therefore the function of memory is less likely to be overlooked. So with very complicated sentences, in which the subject and predicate are built up of a number of terms which are not very commonly associated. Where however the result of the synthesis is already familiar to us as an object of experience, still more when it is so familiar that we have a single term which will represent it, the part played by memory may easily be neglected.

Finally; the term, like the proposition, has very generally been built up from a number of elements. Whereas however the propositions, and still more the syllogisms, indicate this process in their construction and form, the mere term has nothing which prominently suggests it. We are speaking here of course of single terms,—what the old logicians called categorematic terms,—namely of such as are indicated by a single significant symbol; for where they have been built up of a number of more elementary terms, they naturally disclose their composite character and synthetic origin.

Accordingly, in the case of these simple terms for familiar objects, the work which has had to be performed with the aid of memory is very apt to be overlooked. The point to be here insisted on is the fact, already mentioned in the first chapter, that what is often taken for a perfectly simple concept is really the result of an immense amount of synthetic construction. Fragmentary elements experienced at different times of our lives, or even of the lives of other persons, are all swept up together, combined with other elements instantaneously experienced, and retained in a unity by aid of a term. What is actually present to the mind at any one moment, when we make use of a name, is but the minutest fraction of what we probably think to be present, and of what we know to be involved in the use of the name. This cannot be better expressed than in the words of Prof. Sully who has so amply insisted upon these

facts:—"Perception contains not only a presentative element, the actual sensation of the moment, but also a mass of representative elements, picturings of sights and touches." (*Outlines of Psychology*, p. 152.)

III. The two foregoing postulates are very simple and obvious. The reader, if not already familiar with them, will probably be disposed to accept them without further remark. But that which now awaits us is of a far more serious nature, and is one which has given rise at various times to much discussion as to its nature and warrant.

Briefly put, the required assumption is this: that in addition to Uniformity on the part of nature, in the sense already described, there must be a Belief in the existence of that uniformity on our part. We will first discuss the nature of this demand, and the necessity for it, about which there ought not to be an opening for much difference of opinion. We will then go on to the vexed question as to what are our grounds for entertaining it.

(1) As regards the assumption itself it must be insisted on that it is by no means a merely verbal one. Nor is it, strictly speaking a metaphysical one. It is not equivalent, for instance, to saying of the ruler before me, that it must not only exist but that I must also perceive that it exists. What would be meant by this last statement would be one of two things. Either it would be intended to claim that my own perceptive powers are similar to those of other persons; or, more probably, it would be intended to raise the question whether we can reasonably say that an external world exists out of relation to, or at least at a time when not perceived by, any intelligent being. The distinction now before us is however quite different from this, and much simpler and more intelligible. It is indeed one which we can actually see exemplified in an occasional and fragmentary way as things now are: and we can with perfect ease conceive it as being of far wider prevalence.

We can, that is, readily conceive that there should be Uniformity existing throughout nature, and yet that we, or at any rate a number of us, should be destitute of any belief in it; and, conversely, we can equally well conceive that we should be endowed with such a belief though the Uniformity did not exist in nature. And either of these suppositions would be

absolutely fatal to all rational inference and conduct on the part of ordinary beings.

As regards the former supposition, look how matters stand in a game of chance. No one can trace any regularity in the details, considered separately, and probably many gamblers do not believe that any whatever exists there. To *us* therefore such people as these are a case in point. So to the necessitarian must be all the believers in free-will, in the sense of spontaneity. And so to all the rest of us are certain maniacs, at least in regard to their special delusions. It is not therefore very difficult to picture to ourselves the state of a mind destitute of any belief in the uniformity of nature, and utterly unable to acquire it. We can readily see that if such a person were not continually watched and tended and fed by others who were in a more normal condition, it would be a chance if he survived more than a few minutes, and a tolerable certainty that he would not survive many hours.

Similarly with the converse case; that of some person with an unalterable conviction of the regularity of nature but placed in a world where no such regularity existed, and left to make his way there as best he could. The chaos conceived by Milton, or those remote parts of space which Mill maintains that we can at least suppose to be occupied by phenomena subject to no Laws of Causation, would answer the purpose. Put an ordinary present-day physical science student there, and watch him. After he has surmounted the first shock of bewilderment, and passed,—if he ever does,—through the stage in which he is convinced that he has gone mad, he would realize his perfect helplessness. With his sublime but misplaced confidence in the regularity of nature he would quickly get himself crushed to death amongst the wheels because he persisted in believing that the machine would go on working during the next minute as he had seen it work during the one before. But his state, it may be remarked, would not be improved by getting rid of his conviction.

The objective fact, therefore, of the uniformity of nature, and the subjective fact of our belief in it, must be admitted as distinct, though not, of course, independent assumptions. Whether there be a necessary connection between them; and

what, if so, is its nature, will best be reserved for discussion a little later on.

(2) Various expressions are used by different writers in describing this postulate, and something may be learnt from a reference to their statements about it. Each description points to a different aspect of the problem, or to some difficulty which has been encountered in explaining or establishing it. All these expressions concur in regarding the step we have to take, in inductive inference, as being one from the more familiar to the less familiar, but they differ in the words they use to denote it. The three following descriptions are those most in use.

(i) The step from the past to the future,
(ii) „ from the known to the unknown,
(iii) „ from the observed to the unobserved.

(i) The first of these descriptions is the one most familiar to us from the works of the Scotch school, especially the treatises of Reid and Stewart. And it is quite true in the majority of cases that the data of our inference are somewhere in the past, and the fact inferred somewhere in the future. The inferences of primitive man are almost always directed towards the future; and with reason enough, considering the struggle for life in which he is generally engaged, and the small portion of his intellectual force which he can consequently afford to the region of mere speculation. And even, in the case of persons in a more advanced stage of thought and experience, practical considerations are still of overwhelming force and frequency; and such considerations almost necessarily point directly and at first hand towards the future and only indirectly and mediately towards the past. It is therefore practically true that in nearly all inference, and especially in inductive inference, the step we take is a forward one in time: that our acts of inference run, so to say, in the same direction as the actual course of the events themselves.

It needs however but slight consideration to perceive that it is little more than an accident that the conclusions we obtain by a process of inference, whether inductive or deductive, should be future rather than past events. I have discussed this subject elsewhere (*Logic of Chance*) and will therefore touch upon it

but briefly here. The explanation lies, not in any difference between the future and the past themselves, but in the different resources at our service in determining them. To the primitive man, and to all who are involved in practical affairs, the past,—so far as such persons are at all likely to be concerned with it,—is mainly reached by the direct testimony of those who have witnessed the events referred to, or have themselves accepted them on tradition. They do not resort to the more slippery and difficult process of deliberate reasoning unless they are forced to it, and this is not very likely to be the case except as regards the future. But put the perfectly conceivable case that these characteristics should be reversed. Suppose a people with very short memories, or very short lives, with but little intercommunication amongst themselves, and with no writings or other permanent direct records available. To such a people the region of the past would be very much like what the future is to us: it would have to be gained by inference. Suppose also that they had amongst them a race of prophets whose business it was to take account of the future, and who were always open to appeal as we now consult our histories or the memories of old people. Under such circumstances the common attitude towards the past and the future, as regards the points in question, would be exactly reversed. The reader of Dante may remember that such a state of things as this is actually described by him as existing below. The statesmen and cardinals whom he interviews know nothing of what has happened on earth since they quitted it, and they use their opportunity of information in order to correct and supplement the conjectures they had formed. But on the other hand they have some power to predict the future, and here they are able to offer specific information in return. That is, their attitudes towards past and future were nearly the reverse of ours.

Such considerations deserve notice on account of their general interest, and their bearing upon the facts of experience and the conventions of language. That the step of inductive generalization is not necessarily one from the past to the future is obvious enough, the moment we direct attention to the point, and has been repeatedly urged by various writers[1]. The main

[1] For instance Bailey in his *Essays*, p. 199.

reason for such a caution here is that any confusion upon the point in question seems to arise from, and to tend further to increase, a lack of due discrimination between the objective facts of nature and the inferences we draw about them. Nature, as the ground and foundation of our inductions, shows no distinction between the past and the future. We must regard it as stretching alike in both directions, with supreme indifference both to our feeble powers of studying it and to our personal interests in contemplating it. Those powers and those interests make the distinction between the past and the future one of paramount importance to us men as observers and agents, but they cannot transfer this importance into the objective connection of the phenomena themselves.

(ii) Another common way of expressing the step in question is by calling it one 'from the known to the unknown'. In a certain sense this is not inaccurate, but it is somewhat misleading, as it is very apt to raise a wrong issue. It provokes at once a troublesome dispute which we want to defer to a more appropriate place in a future chapter, namely the dispute whether the syllogism is a petitio principii, and whether any new truth can be obtained by reasoning. 'The unknown', in the case before us, is clearly not absolutely unknown to us, or we should not be aware what we had to look for, nor be able to recognize it when obtained. Nor is it, here, something which we had already contemplated as a desideratum, but as to which we had not secured certainty; for the inference to be accounted for is prompt, confident, and inevitable. We take the step in question so spontaneously that it requires at first some effort of thought to realize that any step has been taken at all. In any case the phraseology under discussion could not be admitted without more explanation and restriction than it deserves.

(iii) A third, and perhaps better way of expressing what we have in view, is by describing it as the step from the observed to the unobserved. We must here use the former term in a wide sense, to cover not only what we have personally perceived, but also what we accept on the testimony of the perception of others; and we must take care that this word 'observation' covers all our direct means of acquiring sensible information. The only difficulty which then remains is the one

discussed at the commencement of this chapter, namely that of determining in any given case what *is* observation.

This form of speaking seems adapted to direct attention to the fact that our mode of acquiring or extending our knowledge is to work outwards from the spot where we are; to take for granted what we and others perceive,—all due allowances and corrections being made,—and to step, by inference, from this to what we cannot perceive. The assumption we then require is that the unobserved shall resemble the observed; that the fragments of nature which we have not seen shall be like those we have seen; that nature is, roughly speaking, of a piece throughout, or as Leibnitz says, "c'est tout comme içi."

The above remarks are preliminary to our main subject; the origin of this belief. How do we attain this conviction? What is its value and warrant? Does it first present itself as a general principle, from which particular cases are deduced: or does it start from these latter and find its origin and its proof in the details of the phenomena? The answers to these questions involve, it must be admitted, something of an anticipation and something of a digression also. The question involved here however is one which is so inevitably suggested to the student at this stage, and raises a difficulty which is so repeatedly and strongly urged as an objection against what seems to be on the whole the soundest view of Induction, that it cannot reasonably be passed over without notice. Premising then here, as elsewhere in this work, that the reader has already obtained some slight acquaintance with the nature of the distinction between Induction and Deduction, we will proceed at once to the discussion of an example. The two main points which such discussion is intended to elicit are the following:—

(1) As regards the act of Induction. How is it that an analysis of this operation introduces a difficulty about the origin of our belief in the Uniformity of Nature: and at what point does the difficulty arise?

(2) As regards the relation of Induction to Deduction. How is it that no corresponding difficulty is supposed to be felt in respect of the latter?

These questions can hardly be expected to result in com-

plete answers to our difficulty; but they will, it is hoped, diminish its significance and indirectly point us to the direction in which a solution may be found.

Put then this example. A man is bitten by a cobra. We have known or heard of many other such cases, and they all proved fatal. We conclude with some confidence that XY, the present sufferer, will die; as A, B, C,... the former ones, are all supposed to have died. Here, in these few words we have had all the requisite facts put before us, and we also have the inference from them.

Now since we are looking, in the spirit of logicians, at the existence of this belief, which we know will inevitably arise in every normal mind, we proceed to exercise what Hume calls our "sifting humour", by beginning to press a series of questions. We start by asking the observer *why* he believes in the approaching death of XY? To this question two distinct answers might readily be given. If we were to propose such a question to a variety of persons who had no logical theories to bias the form of their reply, and who had not yet taken sides for or against Mill on "the ground of Induction", it is quite an open event which of these two answers would be given. Some would say off-hand, 'Because every one who is so bitten always dies:' others,—the more wary ones, or those who had some inkling of what was coming next,—would say, 'Because A, B, C,... whom we know to have been previously bitten, have all died.' When these answers are expanded into proper logical shape they would stand respectively as follows:—

Deductive. All men who are bitten die: the man XY is bitten: therefore XY will die.

Inductive. The men A, B, C,... were bitten and died. The man XY has also been bitten. Therefore XY will die.

There is of course always some difficulty in deciding as to what a speaker is to be understood really to have meant, when he gives a summary answer. But the above scheme represents the filling in and expansion of each reply which the persons themselves would probably admit to be the most appropriate under the circumstances.

It need not be said that, in the mood which we are now displaying, we do not rest contented with this preliminary justification. We proceed to put substantially the same

question a second time, by demanding the ground of the inferences: *Why* do you believe that the reason you assign will justify the conclusion you draw? Now it must be admitted that when we ask the "why" of anything, we are asking sometimes for what may lie quite outside the limits of any demand which the logician need recognize as his concern. Such a query is one of very wide import. We may be on the search for some antecedent determining motive, for some prospective design, some physical concomitant, and so forth. But there is not much doubt as to what is intended here. We are asking for some principle or truth which will warrant the conclusion, that is, some general principle of which all similar conclusions shall be particular cases.

Beginning then with our deductive observer, if we pressed him in this way, we should probably find that he would reply, or accept the reply when it was offered to him, that what holds good of a class holds good of every member of that class. That is, he would propose or accept the well-known *Dictum de omni et nullo* of the Aristotelians. And this principle he would probably accept at once in this its widest form: a fact which we shall almost immediately see to be of some significance. It is indeed by no means necessary that it should be accepted at once in its widest form. He might have started by admitting a generalization just wide enough, and no more, for his purpose. For instance he might have substituted the narrower principle that what holds good of serpent bites in general will hold good of each in particular; or he might have given us some wider generalization than this, one referring say to injuries to living or organized bodies. But under the circumstances we should probably get the principle enunciated in the widest form, namely in that of the familiar Dictum.

If we now proceed to press for any further or deeper answer than this, by asking again, Why do you believe this Dictum? the reply would be almost always substantially the same. It would take the form, put into familiar words, that 'we cannot help it.' The exact phraseology might vary:—that the belief was a necessary one: that it was a form or law of thought: that the principle was merely one of consistency, and so on;—but in one way or another we should be reminded that the 'why' with which we prefaced our enquiry in this case had now somewhat

shifted its signification. What it here seems to aim at is not, as in the former case, a mere generalization of the same kind of statement or judgment into its widest possible terms: this has been already effected: and all that now remains open to us is to seek for some description or analysis of the nature of the judgment. When the Dictum was assigned as the ground of the individual inference, all that we were doing was to generalize this latter. When however we are asked for the ground of this Dictum itself, since we can generalize no further, but do not like to fail in meeting every 'why' with a 'because', we are apt to take up a new position and to try instead to indicate or analyse the nature of the judgment. Inasmuch as such a path would lead us outside the province of material Logic we need not here pursue it further.

Now see what comes of pressing a similar set of questions in the case of the second, or inductive form of reasoning; supposing that this had been the line of justification adopted.

We ask our man, as before, Why he believes that XY will die? He replies, 'Because every man who has been bitten has died.' But why does he believe that what has thus happened to every one else will also happen to XY? and so on. The *final* answer will almost certainly be an assertion, in some form or other of words, 'Because nature is uniform.' But though this may be the final answer, it is much more likely that we shall not be landed in it at once, as we were in the previous corresponding case when the Dictum was accepted. We may have a halting-place interposed at such intermediate stages as that 'diseases and injuries run the same sort of course': that 'human constitutions are substantially alike': that 'organized bodies suffer similarly from the same sort of injuries', or so forth. But sooner or later our catechumen will turn to bay at the extreme confines of Logic (as here conceived) by asserting that he believes in that particular fact because he knows that nature is or has been uniform.

So far, the enquiries as applied to Deduction and to Induction seem to run on tolerably parallel lines. We start with the consideration of a single fact or inference and we assign grounds for this, in the form of what may be indifferently termed wider or deeper principles, until we come to some principle which must be regarded as ultimate so far as Logic is concerned. In

the former kind of inference we bottom on a principle which includes, in its express statement, the example in point. In the latter we trust to one which includes, or may include, (so far as past observation is concerned) every available case except that example.

But at this stage we find a difference of treatment in the two cases. The champion of deduction is let alone at this point; but not so he who defends induction. He is assailed at once in the style which Hume rendered so familiar to us. It is hard to improve upon the well known words:—"When it is asked, What is the nature of all our reasoning concerning matters of fact, the proper answer seems to be that they are founded upon the relation of Cause and Effect. When again it is asked, What is the foundation of all our reasoning and conclusions concerning that relation? It may be replied in one word, Experience. But if we still carry on the sifting humour, and ask, What is the foundation of all conclusions from Experience? This implies a new question...."

It is in the last sentence, of course, that the gist of the enquiry lies. Up to this point it seems to me that his account of the matter corresponds substantially to what has been drawn out more fully in the last two or three pages. Various intermediate generalizations are suggested, which may be summed up in "the relation of Cause and Effect"; and the ground in turn of each of these is assigned in the wider generalization which "Experience" affords of all the observed instances in which the law of cause and effect has held true. Understanding the causal relation in a wide sense, this seems nearly equivalent to assigning as the ultimate logical ground of our induction the observed Uniformity of Nature.

Before considering the answer offered by Hume himself to what he very justly terms an entirely new question, and examining whether this will still serve our purpose at the present day, we may pause to see how some of our more popular logicians of recent times undertake to answer the question:—*why* we believe that nature is uniform? or, in the words just quoted, what is the foundation of all conclusions from experience?

Whately,—if we may begin with him,—has a very simple expedient. He merely dismisses the enquiry as irrelevant to Logic. "Whether the belief in the constancy of nature's laws...

be intuitive or acquired and in what way acquired, is a question foreign to our present purpose." In a sense this is true. If Philosophy is to be treated in the spirit of etiquette which we expect on the part of a professional man who declines to step out of his way, or commit himself on any topic which he has not expressly undertaken to treat, we cannot blame the logician who refuses to divulge his views on psychology or metaphysics.

Writers of the Scotch school are more outspoken. They admit that the question is one which ought to be answered,—for that matter, indeed, they are not professing to confine themselves to Logic,—and they answer it in a direct way. They simply postulate an "instinctive law of belief" that "the future will resemble the past" (Reid; *Ed.* Hamilton, p. 199); and again, "This prescience is an original principle of human nature, which I have called the Inductive principle." This belief they consider to exist in its full force from earliest infancy, or even to have possessed more force at that time than it afterwards retains;—a curiously direct inversion, as we shall presently see, of the result which most of the Association school attribute to the influence of experience in modifying our belief. Reid declares himself plainly enough on this point, "This principle, like that of credulity, is unlimited in infancy, and gradually restrained and modified as we grow up" (p. 199). "Children and infants have this belief as soon as they know fire will burn them. It must therefore be the effect of instinct, not of reason." And passages to the same general effect might be multiplied from the works of Stewart and Brown.

It is obvious that this is no *logical* answer to any question which asks a "why?"; that is, it offers no explanation or generalization. We have no right to complain of this, for, from the point of view of those who propose the reply, no explanation can possibly be given. At any rate, if we absolutely insisted on having an answer, we should have to extend the import of this highly extensible interrogative, "why", to somewhat new ground, by admitting, as an answer, the will of the Creator, or some equivalent reason. If He who made the order of Nature in things external to us, has matched this by implanting in the newborn soul a perfect disposition to believe in it, no further answer within the domain of phenomena is needed or is attainable.

For those who can be satisfied with this view, the answer, so far as logic is concerned, seems equally simple and satisfactory both for Induction and for Deduction. In both cases alike we ground at last on a principle which, if sound, does not admit of any further explanation. There is, of course, this difference; that in the one case we cannot even conceive any infraction of the law, whilst in the other we can perfectly well put the case that the instinct should not be true. But the latter explanation does not, any more than the former, admit of analysis. We can neither resolve it into any law more general, nor can we trace its rise or growth. If we accept it there is no more to be said.

The next reply we may notice is that of Mill. Premising—what will come up for full discussion in a future chapter,—that he does not regard the Deductive form of reasoning to be anything but a circuitous way of stating the Inductive, we have of course one and the same explanation to apply to both forms. His reply does not seem to me to be quite satisfactory, at least in the way in which he has himself phrased it.

What he says is that the belief in the Uniformity of Nature is the result of *Induction*. Now Induction, in whatever particular form we may define it, is certainly a process of reasoning, and therefore different from custom or habit, that is, from association in any form. The two may, as we shall presently see, have sprung from the same root, but they represent very different stages of development, as no one has more decisively asserted than Mill himself in other passages. Accordingly he finds himself sharply attacked on the score of inconsistency. 'You appeal',—his opponents are apt to complain,—'to Induction as the foundation of the belief: that is, you make the belief an inference, or the result of an inference: and yet you must presuppose this very belief in each separate act of inductive inference. The first time, whenever it may be, that the man or the child makes an inference embracing a new case, the principle of the Uniformity of Nature is then and there appealed to. That cannot be called a result of Induction which has to be postulated in every single act of Induction, beginning even from the very first.'

This of course is the hostile way of stating the case, but nevertheless Mill's reply does not seem quite satisfactory. It

amounts to this:—I do not presuppose the Uniformity of Nature in the way you conceive, namely as a general principle. For a first act of Induction I only presuppose it in a very limited way: in fact the postulate need not be wider than is just sufficient to cover the particular case before us. And so with each successive induction; no one of them need assume more of uniformity than the minimum required to establish it. By saying that the *general* principle is gained by Induction, I merely mean that we sum up all these separate postulates and so generalize the result into the grand principle of all pervading Uniformity.

This slightly diminishes the logical inconsistency, but does not remove it. For one thing it must be remarked that if Induction is considered to be nothing else than the summing up of a number of separate conclusions or applications of a principle, without extending their range outside the limits occupied by them in the aggregate, we are no longer using the term in the sense employed by the physicist and material logician. We are reverting to the old "perfect induction" of the scholastic logicians. And even so, the difficulty is really left untouched how each of these separate and small applications of the principle is to be justified; for every one of them, however narrowly it may have been appealed to, did most certainly outreach the boundaries of observation as then and there obtained. That is, a uniformity outside and beyond the data of experience must be postulated in every one of those early examples with which we are supposed to start.

Turn next to Hume's answer. In his 'sceptical solution' to the 'sceptical doubts' which he worked out so fully, and from which we have quoted above, he brings us to this point:—"Suppose that any one has acquired more experience, and has lived so long in the world as to have observed similar objects or events to be constantly conjoined together: what is the consequence of this experience? He immediately infers the existence of one object from the appearance of the other. Yet he has not, by all his experience, acquired any knowledge or idea of the secret power by which the one object produces the other; nor is it by any process of reasoning he is engaged to draw this inference. But still he finds himself determined to draw it. And though he should be convinced that his under-

standing has no part in the operation, he would nevertheless continue in the same course of thinking. There is some other principle which determines him to form such a conclusion. This principle is Custom or Habit."

Allowing for slight differences of expression this answer is simply that of the modern Association school. The only point about it to which we might demur is the tone of baffled expectation on recognizing that we are unable to display "the process of reasoning by which we are engaged to draw the inference". It was the attempt to undertake this latter task which led Mill into such fruitless controversy. If one might take the place of counsel, and persuade him to withdraw his own defence and to fall back upon that of Hume, his case would be much stronger.

I am very decidedly of opinion that the difficulty does not admit of any *logical* solution. It must be assumed as a postulate, so far as Logic is concerned, that the belief in the Uniformity of Nature exists, and the problem of accounting for it must be relegated to Psychology. At the same time the question is so inevitably suggested at this stage, and so frequently causes perplexity to students of the subject, that a few paragraphs of explanation and justification must be inserted here.

The difficulty is, really, only one form of that which attends every attempt to explain the actual origin of anything, for it is with an origin that we are here dealing, viz. the first commencement of a consciously held belief. Perhaps the following rather unusual mode of approaching the subject may help to throw some light upon it. Take the case of any one of the more intelligent animals which lead a life in which they encounter a somewhat diversified experience. The dog that has been beaten once will fly from the uplifted stick next time; and if he has received food at a certain house on two or three occasions he will on a future occasion go up to the door with every appearance of expectation and hope. Now so long as we look at him from the outside, we see no difference except in degree between his conduct and that of any human being. That is, we can surpass him in respect of the remoteness of our anticipations and of the intricacy of their conditions: we can see what is likely to happen much longer beforehand, and in much more varied combinations. He lives in a world which goes on with com-

parative monotony, so far as his interests are concerned, and his nature and conduct are in harmony with his circumstances.

Do we find any special difficulty in accounting for these facts, so long as we look at them from the outside? Of course, if we introduce a word like 'Instinct' into the controversy, we may puzzle ourselves to any extent in the attempt to put into it more than it will bear or than we can understand. And it is indubitable that there are whole classes of cases in which the conduct of animals is concerned which are apparently inexplicable on any such simple principles as are involved here. The migrations of birds, to take but one instance out of many, may demand not mere association, but inherited results of parental association, and much more besides than we can understand from any analogy drawn from our own experience. But what we are here purposely confining our attention to is a narrow class of cases closely corresponding to those in which we ourselves act from expectation of familiar sequences. In such cases as these we do not find any difficulty in supposing that association may produce on the part of the animal an immediate unhesitating action such as in our own case we should justify by appeal to a belief. We do not speak of their believing in the Uniformity of Nature, but we find them incessantly *acting* in accordance with it, which, so far as external conduct is concerned, comes to much the same thing.

Now let us conceive that one of these animals, say the dog, retaining all his other faculties as far as possible unaltered, could have a vein of consciousness or self-introspection added on to his nature. And suppose that the first use he made of it was to attempt to answer the question, *Why* do you run up to this door and wait for something: what *ground* have you for your expectation? If he proceeded to answer the question for himself, in the same way as it would have been answered for him by the physiologist or comparative psychologist, he would have to give a very wide answer. He would begin by specifying two or three leading cases closely resembling the one in point; but he would go on to indicate many others with slighter resemblance, and perhaps with conflicting results. And behind all this would lie the infinitely varying experience through which his life had led him and which afforded any general resemblance; and behind that again, presumably, the corre-

sponding experience of his progenitors. All these influences unquestionably *act* upon his belief and are therefore what we may call the 'ground' of it. It is difficult to see what other answer he could reasonably give; for the principle of association looked at from within could (if one may so put it) say no more for itself than could have been said for it when it was looked at from without.

With such an answer as this the mere logician would very likely be dissatisfied. He would probably insist upon something narrower and more determinate. He would say that what he asks for is, not the enumeration of all the antecedent circumstances by which the belief was gradually brought about, but the assignment of the range over which it is consciously generalized. Now such an assignment as this, as we have seen, is almost necessarily indeterminate. Even in the case of the corresponding deductive generalization (i.e. the Dictum) we saw that this, its widest expression, was not so much the generalization consciously adopted in every case, as that which, when proposed to us, seemed the most appropriate. So far as any answer can be given I should think it must be that the extent of generalization will be very variable, according to the circumstances of the case and the temperament of him who judges, but that it will probably not be very much wider than is demanded to justify the instance before us; to which we may add that it seems unreasonable to suppose that the earlier generalizations can possibly be coextensive with what we subsequently accept as the result of our scientific training.

If this be so, we may describe the gradual evolution of the belief in Uniformity, so far as its consideration belongs to the logician, somewhat as follows.

Universal order, or causation, is *acted on* by all men from their early infancy, or at any rate from the first time at which they show any activity of their own. It is equally acted on, in a similar way, by most animals, according to the range of their experience. That is; actions, not merely of the reflex or automatic kind, but such as in our case are of the conscious or purposive kind, are perpetually and confidently performed in harmony with the regularity which exists in nature outside us, long before such regularity is recognized. Anyhow, long

before we have reached the logical stage, that is, the stage at which we can ask *why* we believe, we have already acquired the belief over a number, so to say, of distinct areas of varied but limited experience. It is first appreciated or recognized as a *logical* guide at the stage, whenever that may be, at which we begin to question and justify our actions: or rather perhaps, (in order not unduly to limit the scope of Logic) at the stage at which we might begin to question and justify our actions if someone else prompted us to stand on the defence. Presumably the animals never reach this stage, and man does not reach it until he is some way past infancy; so that it is better not to claim the infant, as Reid and Stewart do, as a believer in Uniformity.

As already stated, this range of conscious justificatory generalization is probably in most cases a somewhat narrow one:—in the example of our snake-bite, it is very likely that all which would be thought of at the time as relevant, or quoted in defence of the inference, would be the analogy of other such snake-bites. By a gradual extension of experience, and a constantly verified appeal to it, this belief is widened in its scope. Although therefore it cannot properly be said that the belief in Uniformity is *obtained* by Induction,—i.e. by a truly logical process,—it may nevertheless be quite true that when it is once consciously realized over comparatively small ranges, all the subsequent growth is fairly describable as being of the nature of simple Induction. By a multitude of such steps, each helping us on a little by extending the appreciation of Uniformity beyond the actual observed case, we may in time gain a complete generalization covering the whole field of nature. How near we approach towards the ideal of realizing an all-pervading uniformity will depend upon our character and the nature of our experience. Those gifted with a strong generalizing disposition, especially if their study of nature has been wide and accurate, so that they have come to appreciate the precision with which remote consequences can be inferred, will grasp it in a very wide sense. Very likely they will hold that such uniformity exists everywhere, extending throughout the whole region of material and mental phenomena. Whether or not they are justified in doing this it seems to me impossible to

say. But it is reasonable to insist that the belief shall become less confident in proportion as it refers to matters more remote from actual experience.

We should see this more clearly if we went more into the details of what constitutes Uniformity. It is, as we have seen, a term of wide import, and by no means coextensive in signification with the causal relation. If, for instance, we confined ourselves to the narrowest and strictest interpretation of the causal sequence, in which, as we saw, the law became almost a formal and necessary one, then indeed it is hard to set any limits to the confidence we should feel in its universal prevalence. But in that interpretation it is purely hypothetical and does not tell us anything about the actual occurrence of phenomena. To postulate, therefore, universal validity for such a law is merely another way of saying that we cannot transcend the laws of our own understanding: that whatever we conceive, or wherever we may suppose ourselves to locate what we conceive, it is still *we* with our present faculties who are conceiving it. Taking the causal relation, then, in this extremely strict sense, I cannot but think that Mill overrates our capacity when he admits the possibility of the law being infringed in the remote parts of stellar space.

On the other hand, when the law is interpreted in the looser sense,—that is, in almost any one of the various subdivisions which were considered in the previous chapter,—Mill speaks with more hesitation than he need adopt. So far from admitting the bare possibility of a breach of uniformity in this sense I should think it not at all unlikely that in the endless stretches of time and space there may be developments in store which fully deserve the name. But as the subject will come under our notice again in a future chapter, we will not pursue it into further detail here.

CHAPTER VI.

LANGUAGE.

On every theory, whether of Logic or of Psychology, Language is intimately connected with all our processes of conceiving, judging and reasoning. Hence it becomes necessary to take some account of this medium of communication. We shall have, of course, to consider it in some detail hereafter under the heads of Names and Propositions. What here concerns us are the more general considerations of its reference, its functions, and the medium through which it is conveyed. We will take these in turn, limiting the discussion, as rigidly as is convenient, to the requirements of the Science with which we are here concerned.

I. In speaking of the *Reference* of language we are alluding to a dispute which may appear to the reader a rather idle one, namely whether the words we use are to be supposed to refer to the objects without us or to the notions within us[1]. It will be remembered that in the first chapter attention was directed to the triple correspondence between these three elements. It was intimated that in a healthy mind these should accurately correspond with each other; in the sense that the same words should always excite, and be themselves suggested by, the same notions, and that either of these should always be understood to refer to the same external phenomena. We explained some of the assumptions demanded to secure such a complete correspondence, and admitted, it need not be said, that nothing approaching to such an accurate fit as this was to be found in practice.

[1] This particular dispute is by no means a purely modern one. Thus, for instance, Smiglecius, when stating one side of the case, says "vox enim, homo, vel Petrus, non significat mihi conceptum hominis vel Petri; sed verum et realem hominem extra intellectum existentem" (*Disputationes* XII. 1, ed. 1618).

Starting then from this basis, with the *word*, the question is asked, Does it refer to,—that is, does it denote, or is it the name of,—the object, or our notion of the object? Popular judgment would, I suppose, decide offhand for the former: the dominant decision of logicians was, till lately, for the latter. Mill, as is well known, held very strong views upon the subject, declaring that the current logical doctrine was "one of the most fatal errors ever introduced into Logic". We shall best appreciate the importance of the question, and see our way to a decision about it, by examining the reasons which may be advanced respectively in favour of the old logical view and against it.

(1) It may be urged, in the first place, that the notion is something comparatively fixed and finite; that is, that it consists of a tolerably rigidly determined group of attributes or constituent elements, which we may conceive to be retained in the mind, or transferred to others like a sort of currency, with ease and security. The objective thing itself, on the other hand, possesses attributes whose number no man can estimate, many of which are fluctuating, others very uncertain, whilst the vast majority are absolutely unknown; and in any case only a very few of them can be present to the mind at any assigned time. We do really know, it may be said, what the *term* 'man' signifies: if 'rational animal' is not enough we can add on more attributes, and come soon to the end of those which are properly characteristic. But who can attempt to enumerate the attributes of *man* himself?

There is something in this; and there would be a good deal more in it if Logic were to be treated deductively, and in the old scholastic style. As has been already intimated (p. 19) the general character of the old treatment was what would be appropriate to a professional class of thinkers dealing with a stock-in-trade of notions whose exchange value was thoroughly familiar to them all. We shall see this better when we come to deal with the Categories and Definition. At present it will suffice to remark that with a well-determined concept-currency of this description, in the hands of men who were in constant communication with each other, and who were much more in the habit of comparing and analysing the notions they had already obtained than of correcting and extending them by

appeal to experience, there was a certain propriety in regarding the verbal symbol as having direct reference to this familiar element rather than to anything objective lying outside it.

But this explanation will not suffice for us. We want to study nature at first hand, or at least to study such appliances for the purpose as are offered by Classification and Induction. We are not like bankers who can get on without fingering anything but currency, but like commercial men who must accustom themselves to handle the actual goods. If one may expand the metaphor we should say that whereas the banker from his point of view need not regard his various paper appliances of credit as referring to anything beyond the coin they represent, the merchant and manufacturer must regard the coin itself as merely a symbol of value, and as therefore having reference to the materials of wealth themselves.

Accordingly what we must do is this. Fully admitting the advantage, for all logical purposes, of dealing with a finite and rigidly determined group of attributes where our terms and propositions are concerned, we must manage to secure this whilst still insisting on the objective reference of these logical elements. The means of securing this will be explained in the next chapter: at present it will suffice to say that what we do is to regard the characteristic attributes as being distinctly objective in their reference,—they are attributes of the *thing*;—but, as being strictly limited in their number, they are the *conventionally accepted group* of attributes, out of the indefinite number of those which actually exist.

(2) In the second place it may be objected that by confining the reference of the name to our notions we evade a troublesome dispute as to what we mean by the 'thing' and its 'existence'. The notion, at any rate, it may be said, is close at hand and intimately known to us. It is there, in the mind, and therefore no speculative doubts can be stirred up about its existence; whereas, when we come to the external object, we not only have to repel the assault of the Idealist, but may find ourselves, after he is disposed of, committed to the defence of the objective reference of such words as centaur and chimera.

This reopens the discussion into which we entered in the first chapter (pp. 28—38), and it does not seem that there is much more to be added to what was there said. Take, for

instance, the centaur. How, it may be urged, can we possibly refer to anything beyond the notion of this creature, seeing that it is now universally recognized that there is nothing corresponding to it in the world of Nature? The reply is not difficult, if we bear in mind the different senses in which the reality or unreality of a notion may be interpreted, and the different standards to which in consequence we may have to appeal. If we were dealing with scientific tests, we should either avoid all reference to the centaur, or we should make it plain that our objective reference *is* to the current notion of that animal. This is no contradiction of the doctrine asserted above. What we should mean is that we were then and there analysing and accounting for the growth of a certain prevalent belief. The notion of a centaur was once widely spread. It was taken up by the poets, and was firmly entertained by those from whom the poets obtained it. To any one of us who use the name at the present day this *is* an objective reference. We are referring to the common notion entertained by others, not to the private counterpart of this notion present at the time to our own mind. This latter may be right or wrong according as it correctly or incorrectly reproduces the former, and this possibility of appeal to a standard outside us is the essential point of what we mean by an objective reference. Similarly, if we came across the name in any modern writing of the type of a fairy tale or poem. I should be prepared to support the objective reference even here, if rightly understood, for we are not now proposing to analyse a once actually entertained belief, but we are all the same referring to a standard outside us which for the subject in hand is recognized to be the ultimate and appropriate test. The writer certainly has in view something beyond his own private notion,—or how should we know what he means?—and he intends to adhere to tradition. Now such tradition is the sole standard or test in this case, so that we may fairly say even here that the name refers to a thing rather than a notion. Provided any ultimate standard of appeal exist outside the speaker's mind, we really have the sort of objective existence which we wish to claim for names and propositions.

The reader will remember that a third standard of reality was recognized in the first chapter; that of mere conceivability. Were this adopted,—as we saw it possibly might in Formal

Logic,—then, it must be admitted, that this claim for the objective reference of our names could hardly be raised. Our subject-matter does not then profess to be anything but what is conceivable: that is, it is a mere stock of notions with nothing necessarily beyond them. But of course, even here, to secure the possibility of intercommunication, it must be insisted upon that the corresponding notions we all entertain shall be identical, that is, indistinguishably similar.

It is best therefore to say decisively with Mill that the reference of names, and therefore of language generally, is to be carried beyond the notion or concept and to be applied to the objects or things from which the notions are derived, or should be derived. We shall say, in language to be fully explained hereafter, that the names *denote*, or refer to, these things; and that they *connote*, or imply, that particular selection, from the indefinitely numerous attributes possessed by these things, which convention has decided shall constitute what is essential in the notion.

The main grounds for this decision will have been gathered from what has been said above in the course of replying to objections. They are two-fold. In the first place, what is absolutely essential for accurate rational intercourse is community of signification. Where the members of a narrow semi-professional class are in constant intercourse it might be possible to secure this by routine or customary usage; but in practice the only safe corrective is to keep a standard of reference before us; and it is therefore best to refer the name directly to that common object, whatever it may be, which we should all agree was intended to be the ultimate standard referred to. And in the second place; even if, as above suggested, perfect community of signification could be secured without our going outside the notion, we should probably also secure what is not at all desirable; unchangeability of signification. We do not want our names to fluctuate rapidly in their meaning; but if they admitted of no modification whatever from time to time, they would not answer their purpose any better than a bodily organ would under the same circumstances. The principal methods for carrying out this process of modification are Classification and Induction, which of course require a constant appeal to the objects at first hand. It is clear that this appeal is more con-

sistent with such an interpretation of the reference of language as is here adopted than with any other.

II. The next point to be here considered is the functions of language. Broadly speaking, these may be said to be, so far as we are here concerned, the three following:—

(1) Its primary object is to communicate ideas from one person to another, or rather, from one intelligent being to another. To enable any sign to come under the strict designation of language, we ought to insist that it shall be deliberately intended to answer this purpose of communication; for almost any outward conduct on the part of a sensitive being gives some indication of what it is thinking or feeling, and may therefore be said to communicate the knowledge to others. The cry of an animal in pain, or its startled movements when frightened, convey to every perceptive being near it, some knowledge of what it is suffering or fearing; but we do not consider such intimations to be language, because the utterances and movements are not intended to fulfil this purpose. On the other hand, to keep to rudimentary indications amongst the animals, the scratching of a dog at the door when he knows his master is within, is distinctly meant to intimate his wish to enter, unless we grossly misinterpret him; and the sort of sneeze which the mountain sheep, stationed as an outpost of the flock, will often give when anyone approaches him suddenly, is equally meant,—so far as we may interpret meaning by conduct,—to convey the notion of danger to the other members of the flock.

(2) The above must be regarded as the primary function of language, in that it is common to every form of it. But there is a second, only less important than this, which is to be found in its capacity to *record* our thoughts. For this purpose it is requisite, of course, that the sign should be a durable one. The name given to this kind of language, it need not be said, is *writing*;—including in this term the various mechanical processes employed to aid or supersede the writing by hand, such as printing, lithography, &c.

(3) The two characteristics mentioned above are far too obvious and familiar to need explanation, but the one now to be considered is somewhat less generally recognized. It is certainly a very important function, for what it implies is that, without

some such system of symbols as that which we call language in the widest sense, all power of acquiring or retaining ideas would be lost. The general fact that this is so has often been recognized, and has indeed found expression in the well-known saying that "language is not the dress but the incarnation of thought". It will be advisable however to work out this truth a little in detail in respect of those two all-important processes of analysis and synthesis by aid of which our concepts and judgments are built up.

(i) Begin with the process of synthesis. Every term which represents anything at all complex helps to bind up together in the mind a number of simple feelings of very different kinds, and coming to us through distinct senses. As our minds appear to be constituted, the employment of an appropriate term to stand for this group is the only way of securing that these feelings shall be so associated together that we can readily reproduce the whole group, or any portion of it separately, according as we please. Take, for instance, the complex term "a wood in spring time", and think of the aggregate of innumerable sights, and sounds, and smells, and touches, which are directly produced or indirectly suggested to us on hearing the words uttered. A certain number of the component elements arise with more or less similarity in all minds on hearing the term, and these are commonly considered essential attributes. It is the indispensable duty of the name to keep these together in the mind. Other elements vary greatly according to the individual experience of the hearer, depending on his temperament, his surroundings, and even on the circumstances under which he may first have heard the word or perceived the object denoted by it. The binding up of these latter characters along with those which are essential, unavoidable as it may often be, must not be considered as an indispensable duty of the name. The fact however that they will often come to be so connected, accounts for the very various associations which we may experience on hearing the same words, and may be the cause, in extreme cases, of actual misunderstanding and fallacy.

(ii) But along with this synthesis there necessarily goes a process of analysis. The former is indeed only another side of the latter. In order to keep the members of the group together each element of it has to be kept apart from other groups with

which it has some degree of affinity and consequent disposition to combine. It is not as if each object involved a group of attributes which it always, so to say, kept exclusively to itself. The same elements perpetually recur in ever-varying combinations. Hence every act of the mind by which we frame a notion corresponding to a term, demands the separation and the keeping apart of many attributes which have so strong an affinity with some of those which we want to retain that they would be apt to intrude themselves amongst these. All aggregation involves selection and therefore separation.

In the case of notions, or groups of attributes, of which we have very frequent occasion to make use, this analytic process is masked, owing to the customary employment of one single word for the notion. Take, for instance, such a simple conception as that indicated by the term "a wood". Not only have all the elements which go together to constitute what we mean by a tree, and an assemblage of trees, to be retained in a group together, but this group has to be held apart from much with which it is commonly associated; from the ground in which the trees root, from the shrubs around them, and so forth. And this exclusion will affect not only the attributes which are always found side by side with the others which we have to retain, but also many which have happened to get mixed up with them in our individual experience. The one word "wood" marks the results of this combined synthesis and analysis; but the latter element is rather in the background, and, receiving no direct aid from the verbal form, is apt to be forgotten. But when we substitute the more complex expression,—at least verbally more complex,—"an old beechwood in spring time", the analytic process is rendered obvious. This particular combination is not a sufficiently common one in our experience to have acquired a single designation; but the fact that we have to adopt a more circuitous plan in order to designate it does not alter its essential character as a name. It is a name still, albeit a many-worded one, and it performs all the functions of a name. But when we select a complex term of this kind we can readily see how each component word in the aggregate indicates a process of analysis. We distinguish the age of the wood from its other characteristics, and in so doing we also indirectly break up the class of 'old things' by detaching the wood from the rest of that group.

Similarly with the very complex elements involved in the word '**beech**'; though, as this word marks a tolerably natural and well-defined group, there is not so much of a breaking up of other groups in order to detach the element we want. As regards the word 'springtime', we make a *time* analysis; we distinguish the wood at that season from the same wood at other seasons. When the whole many-worded term is slowly uttered, with an attempt to realize all its elements, this analytic process is kept under attention; when it is rapidly repeated or listened to, the outlines of this process are blurred; and if ever it became so familiar that it passed before us as a whole, or if a single term came into use to designate it, then the analysis would drop quite into the background. But the essential functions of the name are not altered by these conditions.

Language being thus necessary to any thought of a serious and efficient kind, the question at once arises, How can thought advance? Thought without language we consider to be impossible; and similarly, on the other hand, language without thought corresponding to it would be absurd, for it is quite certain that we do not effect our advance by inventing a word without a meaning, and then imbuing it with one. The difficulty has been sharply emphasized by several writers, but, as in the case of most other statements which are thrown into the form of a dilemma, the distinction is not found to be nearly so serious amongst the facts as it seemed to be in the verbal statement.

The solution is of a kind which is now rendered tolerably familiar in every science which has felt the renovating influence of the Evolution Theory; and we should almost as soon admit, as a real difficulty, that the sap in a plant could not increase unless there was an increase in the number of leaves, nor the leaves increase without the sap doing so. The simple answer is that thought and speech, whether in the individual or the race, advance simultaneously by insensible stages, each concurring to aid the advance of the other: or, rather, one keeps constantly a trifle in advance of the other, the leader, in our case, being naturally the more intellectual element, viz. that of thought. One cannot do better here than quote an admirable illustration of Hamilton[1],—one of those illustrations which in

[1] *Logic* I. 139.

their way play the part of a proof almost as much as of a mere explanation. He compares this nearly simultaneous advance to the process of *tunnelling* through soft and sandy soil. We might raise a dilemmatic difficulty here by asking how the tunnel could have been driven without the brick casing which prevents the soil from crumbling in, or how this could have been built unless there was the opening for it already made. We solve the difficulty in practice by making the two advances simultaneously. We continually scoop out a little ahead of the brickwork casing, which latter follows closely behind and makes each step of advance secure as it goes. So in the realm of thought. New ideas do not spring into existence, and remain as it were unclothed until they are fitted with a suitable word. Whenever any real progress is being made we always find that the thought is straining a little beyond what the accepted stock of words in use at the time was intended to convey. A word already used in one sense is also employed to mark a slight modification of meaning for which we are beginning to feel a want of expression. In time the word is fully recognized as admitting of these two senses. And finally, if we set apart a modification of the old word, or invent a new word,—a thing seldom done unless by borrowing from some other language,—the distinct idea is embodied in its own appropriate word, and takes its place permanently in the stock of words which constitutes the language. In this, and in other ways, the recognized advance of thought, consisting of incessant growth in certain directions and corresponding decay in others, is always at work. But this question belongs to Psychology, historical or comparative. In Logic we must presuppose, as already laid down in one of our postulates, the existence of words corresponding to notions which have already become clear and distinct.

So far, what we have said refers mainly to the use of language in its primary function of acting as a medium of communication from one person to another, but we ought also to take some notice of its employment in aid of private thought. However true it is that language would never have existed but for the urgent stimulus of the desire to interchange ideas, and therefore could not possibly have grown up except in a state of society, there is no occasion at present why it should not equally be put to the uses of private reflection and inference. When we

employ it in this way, whether in the spoken or written form, we generally find ourselves almost as much in a state of dependence upon it as when we are conversing with others; that is, we speak or write to ourselves,—generally the former, because the process, as we call it, of "thinking aloud", is so much more easily practised than the corresponding process[1], as we might call it, of "thinking visibly".

This raises a rather interesting question. When we are thus appealing to a private language, for purely individual purposes, we of course enjoy large liberty as to the choice of a medium in which to embody it. We shall proceed to point out, almost immediately, what a number of alternative resources of the kind are conceivably at our option when selecting a language for common purposes, and how thoroughly practical are the considerations which have excluded all but two of these alternatives; those of speaking and writing. Most of these considerations, depending as they do upon physical and sensible conveniences, do not come into play when we merely want to think privately to ourselves. The question therefore at once arises whether we have not any fresh openings for such an individual language, always admitting that *some* sensible symbol or other is still indispensable?

There can be little doubt that we do make large use of such resources in our private moments, whenever our minds are in a state of activity. It is a point which everyone must decide for himself, since individual experience in matters of this kind is very variable[2]. It seems to me that visible images play a large part in our private reasonings:—images, that is, raised up in the mind, either without being accompanied, so far as we are aware, by any words whatever, or, at most, merely followed

[1] The habit of 'thinking visibly', in the sense of wanting to have the words spelt for us, so as to see them before us, is, I suspect, the besetting sin of the educated classes when learning a new language. We are apt to forget that a word is primarily a sound, and only secondarily a sight.

[2] *How* various are the mental images formed by different persons probably no one suspected until the publication of the very interesting investigations of Mr Galton. See his *Statistics of Mental Imagery*, and *Visualized Numerals* (*Nature*, Jan. 15, 1880; and *Trans.* of Anthropological Institute, 1880). It is only by reading the answers, given deliberately and in all good faith, to the queries he put, that one can form any conception of the difference between man and man in respect of mentally 'seeing' the things about which we are thinking.

by words which were not necessary for the production of the idea. To make it answer such a purpose as this the image must of course be used with a somewhat generalized reference. The true visual image, or intuition as it is commonly called, is necessarily in itself individual, corresponding to the logical 'proper' or individual names. But just as these occasionally become generalized in their application, and thus come to take the place, and perform the functions, of common terms, so it may happen with our individual images. There is nothing to prevent my visual image, say, of some sailor whom I may once have seen, repeating itself with many of its original characteristics when I have to think about sailors in general. And when I am thinking in private I may resort to such a help as well as, or instead of, the sounds or words which I have to employ in talking to others. In fact, when we are speaking slowly on a concrete subject-matter with which we are very familiar, the actual effect of the words we hear is often to raise a succession of images in the mind[1]. This *is* the realization of the language, and there seems no reason why this same effect should not in such cases take place without any words at all.

It seems probable that such a device as this is very common in the case of somewhat ill-educated craftsmen, who are thoroughly versed in their trade, but who have not been in the habit of talking about it with others. And this will serve to answer the question which is sometimes raised, whether, considering the close connection between speech and thought, it is possible that obscure and clumsy expression should be compatible with clear and accurate thought. The answer depends partly upon the subject-matter, but much more upon the temperament and training of the person in question. In the case of the more educated and communicative classes of mankind, with whom speech becomes highly symbolic, such a concurrence is very unlikely. With such persons thought and speech are too indissolubly connected for confusion to exist in one without its existing also in the other. To misuse a mere symbol, unless we afterwards put in a correction, by another corresponding misuse, will almost necessarily lead us wrong.

[1] So at least I find it in my own case. But with Mr Galton's results before us, one has to be cautious about making any one man's experience a law for others.

But with the silent and solitary worker it is apt, I think, to be otherwise. He may vividly realize the action, say, of his machine, and infer accurately what would happen on occasion of any assigned interference with its motion; and he may do this with a certain amount of conscious generalization, perceiving that what he infers about it would hold equally about all similar machines. When one of us literary people, who deal with wheels mostly through the medium of words, hears or utters the statement, 'if the centres are not true, high speed will produce vibration', we are using symbols which we may or may not supplement with images. But when a man of the sort in question is going through the same process of thought he will probably do so mainly by a succession of images, and quite possibly without resort to anything of the nature of a word. This style of thinking and reasoning resembles what we often go through in geometry, when we intuit each step but realize at the time that the same conclusions hold generally and not merely in the example under view. In a complete theory of reasoning this topic would claim fuller investigation, but in Logic, which is understood to deal with communicable reasoning, and has therefore to deal with formulæ intelligible to all who are present, it does not further concern us.

III. The next question we have to consider is the nature of a language regarded in itself, that is, the particular kind of medium of communication which is to be selected. The discussion which follows may possibly seem, to the staid reader, far-fetched and even frivolous. If so, he may be reminded that it is really desirable to ascertain what are the psychological and other reasons why, out of the vast variety of sensible agencies in which language might conceivably embody itself, two or three have been universally selected for the purpose. We take it for granted that the primary function of language is to communicate thought from one person to another; or, in so far as they are able to make use of it, from one sensible and intelligent being to another. For this purpose some system of sensible signs is needed, some medium which can be appealed to by more than one person at the same time. Granted that,—as indicated just above,—any one of us could carry on a process of solitary thought by a succession of images, yet if we are to communicate our thought to others we must know what images

others are thus entertaining, and we must somehow succeed in exciting those images when we wish. This at once raises the question as to how many different kinds of language there may be which would fulfil this general object; and when this has been settled, whether any one of them will answer the purpose better than another.

At a first glance this seems to open out to us a wide array of possible alternatives. Language, we say, is to consist of a system of sensible signs. Now we have five distinct senses, in the ordinary signification of that term, any one of which may serve as an inlet for the sign selected. And as regards the modes open to us for appealing to the senses the choice is practically unlimited, as almost any action we perform will affect some sensible organ of those who are in our neighbourhood. We will take these two different modes of approaching the subject in turn, viz. the sense appealed to and the mode selected for appealing to it. The distinction is important though it is often overlooked: there is, for instance, as we shall presently remind the reader, no absolutely necessary reason why any given language of *sounds* (perceived through the ear) should likewise be a *vocal* language (produced through the throat and lips).

(1) Look then first to the sense through which the symbols constituting the language are to be recognized; laying aside for the present all consideration of the special mode to be adopted in exciting the sense, that is, in creating the symbol.

As regards two of these inlets of knowledge we may make a rather summary rejection. Taste and smell will not adequately answer our purpose. This is not owing, it must be remarked, to any inherent incapacity on their part to recall definite notions by association. Quite the contrary. The adhesive power between the sense-element and the notion is particularly strong in the case of one of these, namely, smell. Everyone must have noticed how the perception of definite smells instantly and powerfully raises in the mind recollections of things formerly connected with them. And this takes place without the slightest previous cultivation of the sense for this purpose, or intentional employment of it in this way, which seems to intimate that in this direction we have one at least of the conditions for a good language. Moreover the range and

diversity of distinct perceptions here is very considerable; it is quite a question whether the number of recognizably distinct tastes and smells is not comparable with that of sounds.

The objections to these senses as inlets of speech-symbols seem to be mainly the following. For one thing they are very liable to variation from time to time, and even to almost total loss. A fit of indigestion or a bad cold would have the effect of rendering us what we may call by analogy blind or deaf. Again these senses are peculiarly liable to disturbance when impressions occur successively. There is something of this sort even in the case of sight; what are known as "after images" being a modification of the present sense-datum by those which had gone before. But for all practical purposes visible images can succeed each other rapidly without suffering any disturbance, in their normal condition, whereas any one taste or smell-perception (particularly the former) may be largely modified by the effects which preceding perceptions have left behind them. Above all, we seem to possess an extremely small power of combining several elementary impressions into practical simultaneity, so as to obtain a total impression of which the parts shall be distinguishable. In the case of sights and sounds we can carry this a long way. For instance, a whole word, in writing or printing, can be gathered up into a unit without the parts merging indistinguishably into each other. We may only visualize clearly one of the letters at a time, but we can practically retain the whole group before us so that they seem to be present simultaneously. This adds enormously to the range of variety of symbols which can be produced, for a few simple signs can be readily built up into a multitude of more complex signs. In the case of smells and tastes we seem to possess scarcely any power of this kind; for I suppose that the pleasure felt by the gourmet in the infusion of a well-adjusted sauce or the cunning composition of a dish, arises either from mere harmony or contrast in succession, or from the production of some new and apparently simple flavour.

There then remain the three senses of sight, hearing, and touch,—including under this last our powers of perceiving muscular resistance and temperature, which are closely connected with touch proper. All these three, but especially the two former, possess in a high degree the main requirements we

demand. Regarding them merely from the point of view of inlets of perception, we must put to their credit the following prominent merits. They are but slightly affected by bodily or constitutional conditions and defects: they offer us a wide range of distinctly perceptible elements: they are sufficiently persistent or recoverable in their integrity for us to be able to grasp up a number of their constituent elements into what is for all practical purposes a simultaneous group: and any one of these perceptions exercises but little disturbing influence upon those in its neighbourhood. So far there would not seem to be much reason to prefer one of these three to another, and in fact all of them are employed for the purpose:—sounds, by all intelligent beings; sights, by all the cultivated and literary races and classes; and touches, by all who are deprived of the other two senses or who wish to supplement their shortcomings.

(2) But this is only one side of the question. Besides the mode of taking in an impression there is also to be considered the mode of exciting it. On this side we have before us not five alternatives merely, but an altogether indefinite number. Our possible means of making noises, producing visible images, tastes, and so forth, are endless. Not only do our own limbs, and vocal and other organs, supply means which are ever at hand, but there are also innumerable artificial appliances to which we might resort for aid. It is the enormous superiority of some of these over others that is the really determining factor in the choice of the particular kind of language to which we are all accustomed.

As regards two of our senses,—taste and smell,—we found that they stood at a considerable disadvantage regarded solely as inlets of impressions. When we proceed to discuss the comparative opportunities afforded us of exciting the requisite impressions, the hopelessness of any expedient of this kind is obvious at once. To produce the same taste simultaneously in every palate in a large assemblage is in itself a matter of some trouble and outlay. But if we wanted to construct a language on such a basis we should have to invent some mode of rapidly changing these tastes from moment to moment. The stammerers' dinner recorded in the *Spectator* (No. 371), at which one gentleman took a quarter of an hour in communicating his conviction of the excellence of the ducks, whilst another

expended as much time in expressing his assent to this statement, might certainly have been better carried out through any other sense; for they probably did not take so long to eat the food as to describe it afterwards. A suitable exchange of dishes, according to some conventional mode of interpretation, would have served greatly to abridge this dialogue; but it must be admitted that the gentlemen in question, by choosing to resort to speech, adopted a mode of communication for which they were unusually badly adapted.

Smells stand on a slightly better footing. It would be debateable whether something might not be done in the way of devising a sort of rudimentary language on this basis for the benefit of any unfortunate persons who were deprived of every other inlet but this. A succession of scents might spell out an alphabet, and a combination of them might even form a nasal image which should be simultaneously perceived by a small company, and raise the same notion in the minds of them all. But fortunately we have incomparably better means than this available[1].

As regards the choice between the other senses, we shall best see the grounds of preference for one or another of them by noticing in turn the main requirements demanded in a successful language.

(i) The first of these requirements seems to be that the apparatus for exciting the requisite sensations in others should be always readily available. In this respect nature has left us but very little option. Our main resource, as every one knows, is *sound*: in fact, this is the only universal, or even approximately general, language. The reason for this almost exclusive preference clearly is the fact of our carrying about with us such an admirable apparatus for producing sounds at will as that which we call our vocal organs. How much of their present excellence they owe to gradual training in the individual, and

[1] If we could ever get inside the mind of a dog we might really learn something on this subject. When I endeavour to recall by recollection the details of a stroll in the country, say by the seaside, they present themselves to my mind as a succession of sights, more or less embellished by a running accompaniment of sounds and possibly of smells. When the dog does the same, on some frequented thoroughfare, or if he could do the same, the events might presumably recur to him in the guise of a succession of smells, with a corresponding accompaniment of sights and sounds.

to ages of exercise and inheritance in the race, does not now concern us. There can be little doubt however that, as far back as we can go, these particular organs must have enjoyed a preeminence for the purpose in question which makes any comparison with other organs out of the question. About the only other methods in which we well could make noises, without resort to any appliances but our own, would be by snapping our fingers, slapping our sides, and so on; as indeed sometimes is done now by way of emphasis. Crickets, as far as we know, have a sort of very rudimentary mode of communication by making noises through scratching their sides. And had a race of intelligent beings gradually become differentiated through this line of descent, they might not impossibly have ultimately developed some highly complex kind of stringed instrument about their own persons, as we may be supposed to have developed our present wind instruments inside our throats. Evolutionary forces care but little from how rude a stage they have to start, provided we give them time enough and suitably varied circumstances.

Our available resources for making visible signs, that is, for appealing to the sense of sight in others, are not quite up to the standard attained in the case of sounds. Much can be effected, of course, with the hands and arms in this way; but in the savage state men have something else to do with these organs and those who took to semaphoring, in order to convey the word of command, would soon be reminded that they had better have been wielding a club or spear. Gesture language is indeed by no means to be despised. It generally counts for something amongst savages, and with some races it counts for a great deal, especially when used to supplement and emphasize vocal intercourse. Again, certain classes of people,—as those employed on railways,—have well recognized symbolic gestures for some of the most frequently recurring emergencies. Thus the arms held straight out mean 'danger', just as unequivocally as a red flag or light, or the use of the word itself.

The only class of people who make habitual and principal use of this mode of communication, and who illustrate how thoroughly it can be made a language, is of course that of the deaf and dumb. Anyone who has watched a party of such persons in the act of rapid discourse amongst themselves will

realize how large are the resources here before us. Indeed, seeing what can be done in this way, the only wonder is that the educated classes at the present time have not found it desirable to cultivate some familiarity with the art of finger speech. There are many occasions on which we want to communicate with some one at a short distance, without calling out, and the special language of the fingers and hands conveying visible symbols would be very suitable. (As we are here purposely confining ourselves to the use of such organs as we have actually about us, we omit all reference to what would deserve notice in any full discussion of the subject: such as semaphore signals, heliography, electric telegraphy, and all the other various artificial apparatus employed for sending visible messages to a distance.)

As regards *touch* also our available resources are considerable. The fingers are as marvellously adapted to convey tactual sensations as they are to receive them, as is shown by the fact that the deaf and dumb can still contrive to talk to each other in the dark provided they are within range of touch. Here again we may take a suggestion from the insects. Had an intelligent and reasoning race of beings worked its way up from the ants or other antennæ-bearing creatures, as great a development of touch language might have resulted as we recognize in the case of vocal language when we compare man with the lower animals.

(ii) The second prominent requisite is that of communication at a distance. This is a requirement which is too simple and obvious to need more than bare mention. The range of difference in this respect between one sense and another is infinite. Beginning with that of sight, which has no limit whatever; we may descend to that of hearing which will extend to a considerable distance; to that of smell which demands close proximity; and so come down to those of touch and taste which require actual contact. In this last case, the speaker and hearer,—if we may use the terms appropriate to the familiar mode of communication,—would have to be within arm's length. The human voice will reach far enough for most ordinary purposes, and, what is equally important, it will continue efficacious in the dark, and can be conveyed round a corner. If we had to cease speaking to our neighbour after nightfall, or when he was

hidden by a bush, language would hardly have been worth developing to any very high degree.

When we resort to artificial modes of exciting sensible impressions we can easily 'talk' at much greater distances. The trumpet and bugle in the army, and the steam-whistle on our railways have been invented under pressure of a necessity to send messages further than the human voice can convey them. Similarly with the flag language of shipping, which can be carried on over a distance of miles; and with the flashing language of the heliograph, which, where mountain heights are available, is only limited by the curvature of the earth's surface.

(iii) A third very important practical consideration is the comparative durability of the organ of communication, and its immunity from injury. Here also the vocal apparatus enjoys a great advantage. To say nothing of accidental injuries, decay and disease offer occasional but very serious obstacles to any reliance on our hands, and are therefore drawbacks to our only ready means of appealing to the sense of sight. So far as gesture language is concerned we are apt to be struck dumb just when our need of help is greatest, namely when we are seriously ill. But so long as the man is alive at all he generally retains some power of making himself heard; and nothing but extreme weakness causes even any considerable loss of power for near communication.

Many other considerations than those above suggested might readily be advanced, but these will suffice to indicate the grounds on which the decision must be supposed to rest, and which have in fact decided the selection. When all the various determining considerations are taken into account,—both those which consist in the relative capacity of the senses to perceive the impressions conveyed to us, and those which consist in the relative power of our organs to excite them in others;—one mode of conveying to others signs of what is going on in our minds stands out with incomparable superiority: that of employing our vocal organs. Consequently there stands out, with equal superiority, one system of sensible impressions: that of sounds. This system is what is recognized all the world over as Language.

IV. A few remarks may be added about the mutual relations of the different kinds of language indicated above,

limiting the attention as much as possible to the kind of considerations which are strictly relevant to a treatise on Logic.

In any cultivated society three distinct kinds of language are to be found side by side. There is the sound-language spoken and heard by all who are not physically deficient; there is the sight-language written and read by the educated classes; and there are two touch-languages handled and felt by those who are deprived of the customary inlets of eye, or throat and ear; viz. one for the blind to read when they are alone, and one for the deaf and dumb to communicate with each other by sight, or by touch when they cannot see their finger signals. Each of these appeals to a distinct sense; and they are all languages in the fullest sense of the term, by which I mean that they are meant to communicate our whole stock of ideas and not merely to be signs of a very limited class of ideas such as the signals used in war, on shipboard, on railways, and so forth.

The enquiry therefore naturally suggests itself whether these three are, or by possibility could be, independent of each other. That is, does each of them stand in the same direct and immediate relation to the notions which it represents, and thence to the phenomena which it denotes; or does one only of them stand in this direct relation, whilst the others are based upon this? We may illustrate by a concrete example. Do the sound indicated by the word *water*; the group of letters 'w, a, t, e, r'; and the set of successive finger movements which the deaf and dumb man substitutes for the latter, all alike refer the mind immediately to the notion, and through this to the substance in question; or do the two latter refer immediately to the former, and only mediately to the notion and the substance? The point is not a frivolous one, for it really raises the question whether there can be more than one kind of language in the strictest sense of the term; that is, whether our vocal organs are naturally the sole vehicle of speech. Professor Max Müller strongly supported this latter view. He maintained that the words of such languages as those of the deaf and dumb, are not signs of ideas, but signs of ordinary spoken words, and are thus one remove further from the ideas.

Historically, there can of course be no doubt that this view is correct. Spoken languages existed as such for many ages before any accessory or substitute for them was ever introduced,

and when the latter did come into play it crept in slowly and followed the lead of the former. We see the process of introduction in its simplest form in the case of an adult learning to read and write. Here every written or printed word symbol, which, in order to perform its functions effectively, should be grasped and interpreted as an indivisible whole and instantly referred to its appropriate idea, has to be slowly and painfully built up out of its constituent letters. Each syllable is separately pronounced, and thus the whole symbol is connected directly with the familiar sound, and thence indirectly with the idea. Substantially the same process has of course to be gone through in the case of children. It should be noticed that the system of building up each written word by the use of separate characters, called letters, enormous as is the economy which it effects in other directions, probably adds rather to the tediousness of acquiring the new language. This must be specially the case with the English language, owing to the extremely arbitrary and variable nature of the syllabic substitutes for the sounds. A word, when consisting of an audible sound, is almost instantly taken in as a whole, unless it be a very complicated one; and the same might conceivably be the case with a written word, for the eye might glance at it and treat it as a symbol whose parts were insignificant. But as a matter of fact, under an alphabetical system, each word is separately spelt out, with the additional inconvenience that the letters have arbitrary names of their own. Many people are familiar with the fact that a comparatively uneducated man is sometimes at an advantage in acquiring a new spoken language as compared with a more educated man, on the ground that the latter will persist even here in trying to spell out the sounds he hears[1], whilst the former is content to take them,—as they should be taken for mere purposes of conversation,—as symbols which he does not stop to decompose into letters.

The merely logical conception of the relations subsisting between the different forms of speech is naturally very different from that which occupies the historian and comparative philologist. Our point of view is very simple. Each separate word, whatever the sense through which it is conveyed, is to be taken

[1] At least it seems to me that, in my own case, this is the principal hindrance.

as an indivisible whole, and referred directly to the corresponding idea, or rather (on grounds already discussed in this chapter) to the phenomenon itself. The spoken word, the written or printed word, and the handled word, are to be regarded as entirely independent but strictly correspondent symbols, each adapted to the condition and circumstances, at the time being, of the speaker and the listener. That is, they are theoretically interchangeable with each other, and nothing but reasons of harmony and propriety hinder us from beginning a sentence with the aid of one set of symbols and concluding it with another. An idea requires some kind of sensible symbol, if it is to be retained or communicated effectively; but it can apparently find an equally congenial home, when once it has become familiar with the place, in any one of several widely distinct sets of such symbols.

Put the following case, for the sake of supplying a more familiar analogy. It is at any rate perfectly conceivable, though it probably never actually occurs. Suppose a person who can speak, but not write, German; and who can write and read, but not speak, English; German written words, and English spoken sounds, being as absolutely meaningless to him as are the shapes of the leaves of a tree, or the song of the birds in a wood. This is perfectly possible. The former condition, in fact, is common enough amongst the ignorant; and to the latter we can find a certain approximation in the case of foreigners who have only learnt our language from books and have never conversed with a native[1]. Such a person would be in possession of two radically distinct languages, appealing to different senses. He could talk familiarly with his own countrymen in German, and he could write familiarly to correspondents at a distance,—or, for that matter, to those in his actual presence,—in English; but not conversely. Such a state of things would illustrate what I conceive to be the true logical conception of Language; as a system of sensible symbols, in the choice and conveyance of which we are prepared to adopt indifferently any organ of sense which will do the work most effectively under the circumstances.

[1] Where the analogy fails is in the fact that such persons would almost inevitably attempt to pronounce, or would 'pronounce silently' in thought, these foreign words, according to their own national conventions of pronunciation. The associations which thus prompt us always to pronounce the words we see in print or writing are quite insuperable.

CHAPTER VII.

TERMS.

THIS chapter has been headed *Terms*, and the choice of this heading seems to call for a few words of preliminary explanation. For all practical purposes we may regard the 'term' and the 'name' as being exact equivalents, and may consider them as corresponding to the 'notion', the 'idea', and the 'concept'; the former being the verbal element and the latter the mental element.

One or two questions reasonably suggest themselves here. In the first place it may be urged that however close and complete the correspondence may be, or rather should be, between these verbal and mental counterparts, yet consistency demands that between elements so distinct in their nature as these, there should be no confusion or interchange. We ought, that is, to adhere uniformly throughout our exposition to one set of technical phrases applicable to one side alone of the duality. There is a decided propriety in so doing, and we shall hope to conform to this usage throughout the more purely logical part of this volume, by systematically speaking of 'terms' and avoiding reference to 'notions'. But there were certain reasons for not doing this in the preceding chapters. In them we were largely occupied with the border ground between Psychology and Logic, and the problems raised were in consequence different from those which now await us. In Logic proper we must suppose that the notion has already become definite, and so far complete; that it requires at most to be fitted on to a name. And the very fact of this assumed close correspondence between the two makes it desirable, for consistency of usage, to keep to language appropriate to one of them throughout our discussion. But in Psychology it is otherwise. We are there concerned

with the notion at first hand, language playing a quite subordinate part. It is the mental aspect which is prominent in the work of recognizing, distinguishing, and indeed creating objects. We were therefore obliged to view the notion as such, in this part of our enquiry, and could not substitute for it any verbal equivalent. But when we have thus secured what may be called our stock in trade of definite notions for the purposes of logical procedure, there is certainly no inconsistency, and there seem to be some advantages, in uniformly treating them through their verbal symbols[1].

We proceed now to consider in turn the principal distinctions amongst these terms or names, so far as they come before us in Logic.

I. *Individual, in contrast with General Terms.* The first distinction of which we have to take account is that between individual and general terms. The distinction itself is obvious enough, and arises out of a universally felt want. Sometimes we have occasion to speak of an individual object, (under the interpretation of individuality already referred to), sometimes of a number of things separately or together. We will begin by enumerating the various ways in which the development of language, acting through popular forms of thought, has found it convenient to refer to an individual object.

(1) There is, firstly, the true logical 'proper' name or term. This deserves its logical precedence because it most completely succeeds in directing attention to an individual object without at the same time conveying any information about that object. The true logical proper name is best conceived as being an arbitrary verbal mark conventionally assigned to an individual, and having no ulterior meaning or significance whatever beyond the fact of its denoting the individual in question. As to what the nature of the "meaning" of a logical term may be, we shall have a good deal to say presently, after we have discussed the distinction between what are called connotative and non-connotative names. We shall

[1] 'Term' is a word of ancient logical usage: 'name' was mainly introduced and popularized by Mill, presumably because it was not a recognized technical word in Logic. As between the other words, 'idea' and 'notion' have done the principal service in England during the last century and a half, or rather since the time of Locke: 'concept' was reintroduced by Hamilton and has been popularized by his followers.

have indeed then to enquire whether any names whatever can strictly be considered to have no meaning beyond that of merely referring us to an object without telling us anything about it. For the present it will be sufficient to offer as instances of what we have in view such a name as 'John Stuart Mill', which, so far as we know (subject to the inevitable exception to be presently made), has not been put to any other purpose than that of marking the deceased logician and philosopher;—or such a name as 'Chimborazo', which with equal definiteness and unambiguity marks a certain mountain in the Andes. Had any one pointed to the person or the mountain in question, and enquired what were their names, no duly informed person would have given any other reply than by uttering these proper names. And had the names been uttered first the reference to the objects would have been equally prompt and decisive.

The only ambiguity or difficulty about such names which need claim a moment's notice is the occasional, not to say the frequent, imposition of the same name to various distinct objects. Thus 'William Pitt' is the name of two great English statesmen, and 'John Jones' may stand for many thousands of men in Great Britain and America. We only direct attention to this because it has some bearing on the primary postulates with which Logic, and indeed the theory of Language generally, must start. The reader will remember that we laid it down in the introductory chapter that all rational communication must presuppose identity of interpretation in the words used. But language being conditioned by human wants it is found quite sufficient if such identity is secured within the limits of ordinary communication at any given time or place. As regards the casual repetition of the same sound in the case of distinct languages there is of course no difficulty. Even if occasional words are the same,—and we must remember that a word or name must be regarded as essentially a spoken and not a written symbol, so that identity of sound is identity all through,—the context removes all possibility of confusion. Now remoteness of time and of place produce for practical purposes the same effect as distinctness of language. Provided that one John Jones lives so long after the other that the contemporaries of the latter are in no likelihood to have remembered the former;

or so far apart locally that those who speak about the one do not see or hear anything of the other, no ill consequences follow. In a primitive state of society, where intercourse is narrowly limited, recurrence of identical proper names causes no ambiguity. If there be a Newport in the Isle of Wight and another in Monmouthshire, people who merely have to walk or ride need fall into no mistake; but those who refer to *Bradshaw* will find need to add on another distinguishing mark, say that of the county. Repetitions of the same proper name must be regarded as theoretically distinct names which happen to coincide. Either they are used in ignorance of the repetition, or, if deliberately retained afterwards, it is because the risk of confusion is not counterpoised by the trouble of making a change.

Whilst on this subject it should be noticed that some logicians have shown a strange and unreasonable aversion to the admission of these strictly proper names. With certain writers,—for example Mansel,—this objection springs from the practice of stopping short at the notion or concept, in our reference of names, instead of proceeding to the external object denoted by the name[1]. My own notion of any individual is of course actually limited in respect of the characters which it includes: what then, it is urged, hinders the repeated recurrence in the course of the world's history of an individual who should present to us exactly the same group of attributes as those which constitute this limited notion? If I could in any way refer back to the standards I could soon put a difference between the two claimants to the name, but having nothing at hand but my limited notions I can detect no difference here. If so, a plurality of objects thus falling under the same notion, this notion is (as we shall soon see) a general one. Individual names are therefore dispensed with on such a theory.

(2) The next kind of singular name to be noticed is that which employs a demonstrative pronoun. Such a pronoun may

[1] "If I say, Cæsar was the conqueror of Pompey, the immediate object of my thought is not Cæsar as an individual existing nearly two thousand years ago, but a concept now present in my mind.........":—the concept having been always understood by him as a bundle of attributes of potentially repeated application. (*Prol. Log.* p. 71.) It need hardly be remarked that this generalization of the proper name is a very different thing from the accidental coincidence of two distinct names noticed in the preceding paragraph.

either stand by itself, as when we say, 'That is a mountain'; or it may be employed to determine and limit a general name, as when we say 'That blue speck is a lake'.

Singular names of this description have often been claimed, and from early times, as being admissible in Logic. It does not seem to have been adequately noticed, however, how widely they depart from the conventions commonly recognized in the case of most other words. Language, broadly speaking, is always understood to be *impersonal*; that is, the meaning of a word does not in any way depend upon the speaker who utters it or on the hearer who accepts it. One of our fundamental postulates claimed that the same signification was to be accepted by every speaker and hearer. This is more or less fulfilled in the case of most words, but there is one class which does not conform to this convention. Demonstrative pronouns and possessives are to all intents and purposes an individual language, in the sense that the same word indicates quite a different object in the mouth of different speakers. My hearer therefore has to interpret it in my sense and not in his own, when I say, 'Give me your knife', or 'I will take this pen'. Accordingly when our power of determining the speaker is lost the meaning of the sentence may be irrecoverable. Write down on paper some statement about 'Snowdon', and though the authorship be lost the meaning of the proposition is in no way affected. But do the same with some statement about 'this mountain' or 'your house' and it becomes absolutely necessary, in order to interpret the proposition, that we should know who was the speaker in the first case, and who was the hearer in the second. Every one who has watched the early attempts of children to speak, has noticed what a difficulty this puts in their way, and how long they are in learning that the name by which others call them is not the name by which they are to denote themselves. Hence the common occurrence of the fact that their own personality, and their own property, are not indicated by 'I' and by 'mine', but by prefixing their Christian names to the sentences they compose.

Admitting, however, the individual and personal developments of language involved in the use of demonstrative and personal pronouns, we see that these thoroughly answer the purpose of denoting individual objects.

(3) Another kind of individual name, or rather a convenient substitute for one, resembles in some respects the kind just considered. It consists in the use of a general name with a conventionally understood individual reference. This reference need not be so narrowly conditioned as in the last case, where it is generally necessary to know the person who speaks before we can know what object he is speaking about, but it is generally limited to the people of a certain date or in a certain locality; and so far it falls short of the complete generality and impersonality which is characteristic of truly general language. For instance, we have not, as a rule, a proper name for our own private garden; but when we talk of going into "*the* garden", our reference is as explicit and definite as if we had such a name. Similarly, when we speak of "the queen", the inhabitants of the country in question, during a given time, are as unmistakeably making a personal reference as if they called her by her proper name. We often in fact adopt this plan, as in this last case, although we have a suitable proper name at hand. Thus, for the sake of variety, we often talk of going to town, or walking by the river, even though the town and the river have well-known names appropriated to them.

It is an interesting point in the history of language, and one to which we shall have to refer again presently, that many of the names which the logician must, in their present acceptation, regard as being true proper names or "unmeaning marks", were in their origin of a distinctly significant description. Thus etymologists tell us that a large proportion of the river names in England have a Celtic source, and originally meant simply 'the river'. Probably many of the names of prominent natural objects, all over the world, which later and more civilized settlers in any country regard as mere marks or proper names, admit of similar derivation. The new comers ask what such or such objects are called, expecting presumably to have its own peculiar name assigned to each. The reply not improbably is that it is called the river, or the mountain, or whatever it may be; and this name, more or less corrupted by the foreigners' attempt to reproduce it, may thus take its place permanently in the nomenclature of the country.

Names of this class seem to me to have a better right to logical recognition than those discussed under the previous

head. Here when we know the speaker, or even in many cases when we know the time and place when and where he lived, we have all the conditions requisite to determine the reference of the name. In the former case, on the other hand, it might be necessary not merely to know the speaker, but to be, so to say, beside him as he spoke, in order that his reference should be intelligible.

(4) The next kind of individual name to be noticed stands on a different footing. It is composed by building up a substitute for a true proper name by a combination of two or more general and significant names. This course is often adopted for the sake of variety, or for rhetorical effect, but there are some cases in which it seems our best or even our only plan.

In the first place the individual object which we wish to distinguish may not possess any proper name of its own, or we may not know this name if it does possess one. If we are not in a position to point it out demonstratively our only resource may then be to discover such a combination of descriptive terms as shall isolate this object by being applicable to it alone.

Or it may happen that the individual is not at present identifiable, and then there is hardly any other way than this of determining him. By 'not identifiable' is meant that we cannot, so to say, put our hands on him or point him out; we only know him through some action or some relation in which he stands to something else. The original inventor of the mariner's compass, and the murderer of Sir Edmund Berry Godfrey, probably had their own proper names; but in the entire impossibility of determining how they were called we have no other way of alluding to them than by thus indirectly indicating them. It need hardly be said that such an "unknown quantity" as this, to borrow the mathematical phrase, may quite fairly be an object of discussion. It may be known for certain that some one person must be the author of a crime, but until we have detected him we can only refer to him in this descriptive way.

Or, again; there may be special reasons for calling attention to the characteristics of an object which are intimated by certain significant terms, because we wish to emphasize these characteristics, which its proper name would fail to do. The victor at Waterloo, and the premier who so stoutly opposed the Catholic

Relief Bill, may be one and the same person as the first Duke of Wellington, but there may be special reasons for substituting in certain cases one or other of the former designations in preference to the latter. There is a considerable difference in the total amount of information and suggestion yielded according as we predicate of a person under one or other of these designations that he thought it necessary to take serious precautions against the Chartist gathering of April 10, 1848.

As regards these various devices for isolating an individual by a combination of descriptive terms, an important restriction must be made. Strictly speaking, no such process can really narrow down the reference to one individual. As we shall presently see, it is of the essence of a true general name to have an actual or potential application to an indefinite number of objects. A combination of such names will certainly curtail the range of application, but it can no more restrict us to a single individual than successive subdivision of an area can restrict us to a mathematical point. In fact these substitutes for truly individual names can only answer their purpose under certain tacit assumptions. They presuppose limitations and conditions of time and place; which being granted, it may happen that only one object will answer to the description. But wider knowledge on our part, or a change in the circumstances concerned, may render the application no longer determinate.

(5) Again: in the last group the limitation to an individual object, so far as it was attained, was comparatively accidental. In contrast with this we may secure a *formal* limitation, whilst yet employing a group of descriptive or general terms[1]. There is, in fact, a class of general names of a quantitative or numerical character which entirely escape all ambiguity of the sort in question. The words 'first', 'second', and so on, are true general names, inasmuch as they are applicable to any succession of numerable things. So with terms indicative of geometrical position or magnitude. The last poem written by Byron; the Westernmost island in Great Britain; the tallest man in Europe, must be reckoned as truly significant names, but they are all, by their very form, strictly singular. It is

[1] General, that is, in respect of *some* of the constituent elements; but it may be doubted whether, as in the examples which follow, we have not to help out the individualization by aid of a proper name as well.

quite possible that many of those who use these names might not be able, so to say, to put their fingers on the objects in question, but this is a contingency applicable to all singular names. We may use them intelligently and correctly without being able to discover for ourselves the objects they denote. How many, for instance, of those who speak of Rudyard Kipling or of Wenham Lake could undertake to go out and unhesitatingly point to the person or thing which bears the name?

(6) There still remains one class of truly singular names, viz. those which used to be called "individua vaga". In this case, as in that last alluded to, we limit a general name arithmetically, but in a way which, assigning no place or order to it, makes no attempt at identifying it. For instance, instead of saying 'the first king' of any country, we say 'a king', that is, one indeterminate king.

It may be asked, What can be the use of thus predicating something of an individual who is not merely unidentified, but possibly unidentifiable? It must be replied, as a rule, that there can be very little use indeed in doing this, unless it be as a step to something beyond. This will come out more clearly further on, when we come to deal with propositions. We shall there find that these vague individuals enter into our propositions mainly in the following ways.

(i) As contradicting a universal assertion. (We shall hereafter see that one special function of all particulars is to put a check on universals.)

(ii) As a first step towards establishing a universal. The mere knowledge that *a* man has recovered from cholera is valuable as denying that none do; and though it does not lead to the conclusion that *all* do, it may lead to a specialized particular,—that is, to a narrower universal,—which may give valuable information.

(iii) As a first step towards more accurate knowledge, even though there be no opening for generalization. 'A man was murdered last night': clearly the next step is to identify him if possible.

(iv) Propositions of this wholly vague kind present themselves, as we know, as results of the syllogistic process.

All the above six classes of names are truly individual; that is they are employed to mark out a single object. The first of

them,—commonly known as *proper* names,—are, as indicated, to be regarded logically as purely unmeaning marks put upon the objects for purposes of reference and identification. In order therefore to use the name with full intelligence we ought to have had the object, at some time or other, actually pointed out to us. The remaining five give some description of the object. They may either carry their own interpretation along with them, or they may need to have certain determining circumstances of time and place indicated in order to make their reference definite. The last of them does not profess to be definite at all.

II. *Collective Terms.* The recognized characteristic of these is that they denote 'a plurality of objects regarded as a unity'. But this at once opens up a difficulty which deserves notice. The reader who bears in mind the remarks in the first chapter will remember that when we talk of a 'single object' we are taking a good deal for granted; for the unity which we attribute to it is in great part of our own creation. There are no doubt some cases in which the recognition of unity is forced upon us in a way which it would be absurd to raise a quibble over,—as, for instance, the case of human beings, and most of the objects to which we attribute life,—but on the other hand there are many unities which have become such owing in some degree to our own choice, and which are therefore of an artificial character.

Now, as I apprehend it, the distinction thus indicated corresponds in great part to that between the Individual term and the Collective. In certain cases the constructive unity indicated by the name generally resolves itself, when broken up, into heterogeneous portions, or into such as are less commonly recognized as unities. Thus, 'Blenheim' (house) may be resolved into rooms, passages, roofs, staircases, and so forth, all which are very distinct objects; and 'Snowdon' resolves itself into certain arrangements of slope and cliff, &c., which we had probably never consciously regarded as objects at all. These, then, are not collective names. On the other hand our constructive unity may readily break up into an assemblage of objects which are universally recognized as unities themselves, and which indeed may have been perceived as such before we thought of aggregating them into a whole. Thus

an 'army' is obviously made up of a multitude of men of the kind individually familiar to us as soldiers; the 'Milky Way' has now become a collection of objects, for though it acquired its name when it was supposed to be truly singular, we now know that it is, like a constellation, resolvable, and consists of a multitude of stars. These constructive unities, thus consisting in general of a number of similar objects, are what are known as Collective terms.

They are of very various kinds. Sometimes they are unique, —unique, that is, as an aggregate,—though their constituent elements are numerous. Thus the House of Commons consists of all the persons known as 'members', or by whatever name we call them; and it is, at any given time, the only thing in existence which is generally known by that name. Sometimes, again, these collections may themselves be numerous; thus there are many 'regiments' each consisting of many soldiers with the same uniform and equipment. And these collections may themselves be subordinated under a higher collection. This subordination is most completely carried out in the province of Natural History, in the so-called classificatory sciences. 'The Ranunculaceæ' is a collective name for certain groups, such as aconite, &c., each of these being resolvable into similar groups, whilst it is itself an element in the broader collection known as Dicotyledons.

One peculiar case which deserves somewhat special notice is that of *substances*. Take, for instance, *gold*: Do we regard this as a unity or not? that is, does the name stand for all existent pieces of gold, regarded as a mental whole, so as to constitute a collective term; or does it stand for all such pieces distributively, that is, are we able to predicate it of each separate piece?

Some confusion and difficulty have been experienced about these 'substantial names', as they have been termed. My own view is that they are of ambiguous import, admitting sometimes of one and sometimes of the other of these significations. When they occupy the place of subject in a proposition they seem to be of a collective character. Thus when I say 'gold is heavy' I am thinking of it, or at any rate referring to it, as a whole; or perhaps, more strictly, am referring to a piece which is consciously regarded as representative of all the rest. And this

seems the most natural and appropriate usage; for when we refer to any of the separate elements which compose the whole we generally, by the form of our sentence, make it plain that we are treating them as parts of a collection. We speak, for instance, of "a piece of gold". When, however, the term stands as predicate of a proposition it appears to partake rather of the distributive character. I say, for example, 'This is gold', whereby I treat the term as a general adjective, just as when I say 'This feeling is human'.

The reason why terms which denote a substance should show this peculiarity is not difficult to see. It is to be found in the special divisibility and homogeneity of such a substance, which make it almost impossible for us to adhere to our ordinary conceptions of unity in reference to the constituent parts. The different pieces of gold which form our coins and rings stand on a totally distinct footing from the separate persons who compose a crowd, or even from the separate stones which compose the class of diamonds. The fact that we can divide and reunite as we please, and take any one piece of gold as a fair specimen of any other piece, confers an obvious unity upon the whole assemblage of pieces such as can hardly be found elsewhere, whilst it offers great difficulties in the way of our regarding any single casual piece as in any strict sense a unity.

It may be remarked that there are linguistic devices for contemplating and designating most groups of similar things in both this collective and distributive way, and the variety of such devices deserves a moment's attention. In such cases as those of substances, mentioned just above, where the component elements are very homogeneous and any portion is divisible at pleasure, the term is primarily collective, and the separate component portions are indicated in a derivative way. Thus 'salt' is a collective term, and when we want to mark any of the portions which, if they were naturally and permanently more distinct, would be regarded as individuals, we have to speak of "a piece of salt". In the bulk of cases, where the individual objects are tolerably distinct from each other, and have obtained names at an early period, the distributive term is apt to be the primary one, and the collective term is an adaptation from it. Thus we have a name for "birds", distributively, but if we want to make a whole of them all we

must speak of "the species of birds" or "the bird family". There are again certain cases in which the things come before us frequently and familiarly in each of these relations, and where in consequence we have a pair of suitable names. Of these however one is generally a direct derivative of the other as 'mankind' of 'men', and 'the electorate' of 'electors'.

The reader will observe that the collective names we have been taking account of belong to one or other of the kinds which may be called significant, as having a meaning. It may fairly be enquired whether there are any collective names which are, in their present acceptance at least, purely arbitrary marks assigned for the purpose of distinction to the group in question? In other words are there any *groups* of distinct things which have purely *proper* names?

There are such names to be found, but they are not very common. One instance of them is exhibited in the case of geographical groups. For instance, the Seychelles, and the Pyrenees, are distinctly, in their present usage, proper names, denoting respectively two groups of things. They simply denote these groups, and give us no information whatever about any of their characteristics. In this case such groups were probably first appreciated,—as they still are by all who contemplate them from a distance,—as a whole rather than in their parts; that is, their character as a unity was prior to, and more prominent than, their character as a collection. It may be asked whether the converse case is possible; in other words, whether we can take a number of objects and impose a true proper name upon them as a group or collection? Certainly we can do this. There is nothing to hinder us from taking a 'scratch lot' of things, to use the slang phrase, and giving a name to the selection with the caprice which we show in naming a yacht or a dog. The various persons who happen at any assigned moment to occupy a given space in Fleet Street, or the topics of conversation in some particular ball-room, may if we so choose be regarded as a whole, and have a collective name of absolutely arbitrary character assigned to them. To do this would however be frivolous. In imposing names we must have some regard to the exigencies of life; and as every fresh name is one more competitor in the crowd of names which are already struggling to find or retain places for themselves, we do not

impose them except where we expect to have tolerably frequent occasion to use them. A merely casual assemblage of things is obviously one with which we are not likely to have frequent need to deal, and it is therefore waste of resources to assign a proper name for the exclusive use of such an assemblage.

We have now discussed, under two general heads, the principal ways of naming a single object:—firstly where the unity was the prominent point; and where, though the object might admit of resolution, (as it almost always must), yet such resolution was thrust quite into the background, or completely disregarded. These form the class of *singular* names, of which *proper* names are the most characteristic class. Secondly there was the case in which the unity was one of two coexistent aspects; where, though we grasped the objects into a whole, we were well aware at the time that there were many objects to constitute that whole. These, though really a particular kind of singular name in a great many cases, are best separated off into a special class. They constitute the so-called *Collective* terms. This leads us on naturally to a third class; that in which the plurality of the objects is the prominent point, and where any unity they possess as a group, is indirect and comparatively disregarded. These form the bulk of what are called *General* terms, and must now occupy our attention.

III. *General Terms.* What we want to do here is to find a name which shall equally well fit, that is, which shall be the name of, any one of a number of objects. There is practically only one way of doing this. We impose a name which has a meaning, that is, which implies certain attributes, and if these attributes are found in any object then we consider that this object is marked by the name. The reader must observe that the conception of a unity in respect of the group of objects denoted by the name, though it has sunk into the background, is not lost. The mere fact that we regard the objects as belonging to one class, and that they are bound together by the common link of a name, confers a unity upon them. This meaning of names is called their Connotation, or Intension, and names with a meaning of this kind are commonly known as connotative names. The extreme importance of this characteristic, and of the distinction which it introduces,—perhaps the

most valuable distinction for purposes of intellectual profit to be found within the field of Common Logic,—must be our excuse for devoting a considerable amount of space to its illustration. Directly we begin to speak about names having a meaning, we see that this sets before us two sides of the name, that is, two aspects under which a name may be viewed. These are respectively its meaning and its range of application; or in other words the characteristics which it is meant to imply and the objects to which it is found to apply. In technical phraseology these are known as the connotation and the denotation, or the intension and extension, of the term. The best way perhaps of bringing out their several characteristics will be by the successive discussion of a certain number of points.

(1) It is obvious from the mere statement of the relation between these two elements that they must to some extent vary inversely with each other. The more meaning we insist upon putting into a name the fewer will be the objects to which that name will be appropriate: the less the meaning contained, the wider will be the range of application of the name.

It need hardly be insisted on that any such strict relation as that which the mathematician understands by the term 'inverse variation' is out of the question here. If we double the meaning,—so far as this expression is intelligible,—we certainly do not halve the extent or the number of objects covered by the name. The utmost we can say is that, as a rule, the more the connotation the less the denotation, and conversely. But even this statement is only true with exceptions. Assuming that the denotation is to be in any way actual, and not merely potential or conceivable,—we shall proceed presently to draw out more fully the significance of this condition,—the two elements will not by any means vary uniformly and continuously together. The attributes of things as found in nature have a habit of adhering in groups, in the sense that the whole group is present or absent together. It is upon this fact that the main significance of what is called a Natural System of Classification depends, for by securing one such attribute we indirectly secure a number of others also. Thus, the properties indicated by the terms 'exogenous' and 'dicotyledonous', though distinct are always found to be associated, so that the predication of

either of them necessarily involves the applicability of the other. Accordingly the predication of both together does not narrow the application of the term more than the predication of one only would. Hamilton used to exhibit to his class, by way of illustration of the mutual relation of the connotation and denotation, a cone with its vertex upwards. Like all his appeals to mathematical conceptions or illustrations this is misleading. In a cone the two elements, namely the height from the base and the breadth of the section, vary continuously: what he had better have chosen was a figure in which, instead of a continuous slope from bottom to top, there was a succession of steps each leading by a slope to the next, so that the variation of the two elements should be discontinuous.

This suggestion however raises a point which deserves enquiry. If you diminish the width of the section of the cone sufficiently, by cutting it higher, you at last reach a mathematical point. By analogy, if you keep on adding fresh determining attributes do you limit the application of the term down to an individual? That we can practically do this, has been already admitted; in fact this course is sometimes adopted when we want to indicate an individual without calling him by his proper name. It was however insisted on at the time that such limitation to an individual is precarious; it depends upon implied conditions of time and place: no combination of general names can ever yield anything but what is still strictly a general name, though we may happen to know that here and now we may safely employ it to designate some particular individual. The only way of correctly designating such names is to describe them as connotative names which are practically singular, that is, singular under existing circumstances. To confound them with those truly singular terms known as Proper names would be a great error.

(2) We must next say something about the *nature* of these two aspects of a term: What kind of things are they? To what order of existences do they belong? The statement that they vary inversely with each other might suggest some sort of homogeneity between them, which is far from being the case.

The nature of the Denotation is plain enough logically. It simply comprises the objects themselves to which the name is

applied. The reality of these objects must always be presumed, but the nature of this reality,—as already fully explained,—will vary according to the subject-matter with which we are dealing. The denotation of 'horse' comprises all the animals which go by that name. The denotation of 'ellipse' comprises all the curves which the mathematician can conceive answering to the law of such curves; that is, the range in this case is only limited by our powers of conception. The denotation of such a term as 'Griffin' is open to some difficulty: but, in accordance with the principle of interpretation laid down in a former chapter, I should say that we must seek the beast and its denotation in their appropriate home, that is, in the tales of the poets and on the seals and coats of arms recognized by the heralds. The principle of selection in all these cases ought to be easy enough, however difficult it may practically be to carry it out. The rule should be simply this: in order to ascertain the denotation of a name, summon up from the realms of fact or of fiction, from the actual or the conceivable, as the case may be, whatever answers to the name; that is, whatever when pointed out to us we should admit to be marked by the name.

The connotation, on the other hand,—unlike the denotation which is real,—is notional; or rather it involves this characteristic in a much higher degree than the denotation does. It is something conceived in the mind, and only realized by abstraction. The best summary account is that the connotation comprises the attributes marked by the name. Thus, to take a stock old example, 'man' *denotes* all the individual objects which we will call by that name: it *connotes* the attributes of animality and rationality. These attributes are not things which we can point to, and group and separate at pleasure, but they are creations of the comparative faculty. They are points of agreement amongst the material objects which we detect by comparison, and retain by aid of the abstract names which we use for them. Whatever synonyms we employ to signify them, —qualities or attributes of things, points of agreement, or what not,—they represent the subjective side of Logic. The denotation we may be said to find; the connotation we must be said to make.

(3) The next point to enquire into is the relation of these two aspects of our terms to each other in respect of their

priority: that is, which of the two must be considered rightfully to take the lead and thus to determine the other?

This question is one which would probably not have occurred to the older logicians; or, if it had been noticed, would have been regarded in a way very different from that which we now adopt. With them, the connotation (under whatever names they indicate this quality) was far the most important and easily determined; the denotation interested them comparatively little. We shall have plenty of illustrations of this from time to time; but it will be sufficient here to note the confidence with which definitions of terms were offered, and the unanimity with which they were generally accepted. This is very much what we might have expected on the part of those whose knowledge of the facts of external nature was comparatively slight, and whose love of consistency was great.

It must be admitted that to a considerable extent they were correct in their view. The connotation is by strict logical right the primary and determining element. For let this be definitely assigned, and the denotation becomes at once,—so far as our powers of observation permit, and with due assumptions as to the field over which our enquiry is to be considered to extend, —capable of determination. But the converse does not hold good. Assign a denotation, and there is no corresponding necessity in determining the connotation. Of course when we have a group of objects set before us, we can proceed to ascertain what attributes these objects possess in common; but this, as we shall presently see, is a very different matter from determining the true connotation of the name which is to denote that group. No mere inspection of a group of objects can enable us with confidence to assert what was the principle in accordance with which they were selected.

In practice, as we shall find when we come to the consideration of Definition, and especially of Scientific Definition as distinguished from that which finds a place in Formal Logic, these two qualities or aspects of a term are employed mutually to determine each other. It is one more instance of that alternate 'give and take' by which our knowledge is in almost every direction progressively built up. We must always be prepared to modify the connotation of a term, as it has been hitherto currently accepted, in order to enable it with more

propriety to include objects which popular usage regards as belonging to the same class; or even to make from time to time alterations wide enough to enable it to include objects which are allied with those already included. On the other hand, it is obvious that we must always be ready to revise the commonly accepted denotation, in the way of accepting or rejecting such and such a claimant, in accordance with the current connotation. In fact we demand a certain amount of mutual concession on each side, in order to have as little and as gradual disturbance of meaning and of application as possible, with due preservation of consistency.

(4) The next point for discussion concerns the range or limits of these two elements, the denotation and connotation. We will take them separately, beginning with the former.

As regards the Denotation the principal speculative difficulty in this respect is one which has already confronted us; and which, in a science like Logic, which deals with questions of truth and falsehood under their most general aspect, cannot but confront us repeatedly. The moment we begin to answer questions as to what range is included in the denotation, we are in fact called to put an interpretation upon 'reality' and 'existence', and this is an interpretation, as we have already seen, which must be regarded as susceptible of considerable latitude.

We will begin with an extreme case in one direction, namely with objects whose *esse* is *concipi*; for instance with mathematical figures or formulæ which need not have any other kind of existence than that of being pictured or thought. Here it seems to me that the denotation, if we are to speak of such, is potential only, and must be held to embrace every ellipse, say, which ever has been or will be imagined. For any such figure will answer every purpose which can be demanded of it in the way of furnishing a starting-point from which to deduce the properties of the curve. The statement that ellipses were present to the mind of Euclid but not to that of Thales, is presumably verifiable by evidence, just as is the statement that there were crocodiles in Egypt but none in Greece. The figures which any ancient geometer may have pictured to himself are as much a part of the denotation, that is, are as true and real ellipses, as any which I may draw on the paper before

me. This, I take it, is the only answer we can give if we are driven to apply this character of denotation to such entities as these. Such an application seems however decidedly far fetched. The conception of Denotation becomes appropriate only when we are concerned with objects whose existence is limited in some material way. When, as in the case before us, we are dealing with mere notions or images, and their conceivable relations, there seems no valid ground for introducing such a technical term at all. Consequently the whole doctrine of Denotation seems a mere excrescence in the systems of logicians who, like Hamilton and Mansel, make logical existence for the most part a matter merely of conceivability.

Now turn to the opposite extreme, by considering the case of things whose obvious business it is to be real rather than notional. Take, for instance, some creature which never having played a part in fiction or fable may be considered to belong to the province of the zoologist and natural historian. If the creature is still to be found anywhere we may say that its denotation, unless otherwise stated or implied, may be held just to comprise all living specimens. If it is now extinct, like the Dodo or Moa, a perplexity arises which has never been judicially decided. Perhaps it is best to introduce a reference to the element of time, and to say that it has now no denotation, or that its denotation is no longer represented amongst living things.

As regards those existent classes of animals which have, so to say, a literary as well as a physical life, further perplexities may easily be stirred up. For instance, are the foxes of Æsop a part of the denotation of that word? Do Rosinante and Bucephalus take a place amongst the examples of the horse? The only answer we can give is one which has been given already to analogous enquiries, namely that we must take into account both the speaker and the context. A writer on Romance or early art who should maintain that all horses belonging to knights were well-groomed might fairly be opposed by the instance of Rosinante; but a zoologist who wished to know whether wolves and foxes can pair would not serve his cause by appealing to Reineke Fuchs.

The case of things which are now notoriously fictitious affords some interest from the side lights which are cast upon

the historic course of experience and belief. Such things as dragons and griffins are not meant to be conceived at will, like geometrical figures which owe their reality to our powers of intuition; nor are they meant to be tested by current experience, like the data of Zoology. The fact is that being 'survivals' they will not fit in with our modern technical phraseology. No doubt to the mediæval logician the dragon was as real as is the walrus to most of us. Had he been sent to go and seek one he would have started for the wilder mountains with much the same expectations as a Russian peasant in search of a bear or wolf. He could see pictures of them: he had spoken to many who claimed to have seen them: nay, corresponding to the tusks of the walrus, were there not material relics[1] of dragons to be inspected in the museums? Entities once believed in die hard: for ages after they have been dispelled from scientific works they linger in popular belief: and for ages longer, if not for ever, they will survive in the regions of fiction, heraldry and romance. But they fit in badly with our technical phraseology. The best answer we can offer in respect of the things which we now take to be fictitious, is that, in order to find their denotation we must just go to the romances, the accounts of early travellers, the classic poets, illustrated copies of old works, and such like authorities. The sum-total of such references as these may be regarded as constituting what "experience" offers in this line, and as therefore comprising the denotation of the term.

The reader must be careful not to confound the Denotation with what is sometimes called the Universe of Discourse. This latter term was introduced by De Morgan (*Formal Logic*, p. 55) but like some other supposed modern introductions the distinction implied by it had not escaped the acuteness of the early logicians[2]. What the phrase is meant to indicate is, not the whole range of objects to which a general term can be correctly applied,—this is the denotation,—but merely the restricted range to which the speaker at the time being intends

[1] According to Sir L. Stephen (*Playground of Europe*, p. 55) the remains of one were preserved in the museum at Lucerne. They had probably been found in the neighbourhood of Mount Pilatus.

[2] Much discussion on this subject will be found in some of the old treatises under the head of *Suppositio*.

his remarks to apply. It is obvious that we often use general language when we have no intention that it should be taken in its full generality. The conditions and limitations may be of various kinds: of time, place, circumstance, and so forth, but they generally exist to some extent and are fully recognized in practice. That this should be so is clearly a departure from stringent accuracy, and we try to avoid it in our scientific communications. But popular language is highly impatient of the explicit introduction of limitations whenever they can be safely taken for granted. Accordingly it prefers to make its statements broadly and in the fewest words, only inserting such qualifications as would not certainly be supplied by the hearer.

To this narrow conventional denotation of our terms the name of Universe of Discourse has been given. Its consideration does not strictly belong to the province of Formal Logic, and but for the convenience of explaining the distinction here in connection with Denotation, the topic might more appropriately have found its place amongst the Prolegomena of our science. We might, for instance, have inserted, amongst the assumptions of Logic, the claim that the speaker and hearer should be in agreement, not only as to the meaning of the words they use, but also as to the conventional limitations under which they apply them in the circumstances of the case.

(5) We now turn to a similarly detailed consideration of the nature and limits of the connotation of our terms. We may begin with laying it down, as already suggested, that the connotation consists of the sum-total of the attributes generally recognized to be implied by the name. But this statement opens the door at once to a number of objections and queries. As a fact, do any two persons really mean the same thing exactly by the words they use? Does even the same person at different times? Such objections have some force unquestionably, but they come a little late here; for they might clearly be raised, not against the consistency of Logic only but equally against that of any other science which deals with language. It may just be remarked that the first step in the way of the requisite admission must already have been made by the objector himself; for no one can even raise a difficulty verbally without presupposing that every significant word means the same to him-

self that it does to his hearer. Common intercourse can doubtless be carried on with but a slight consensus of this kind,—it is in fact itself the process by which our notions are formed and rectified,—but a science such as Logic must presuppose more than this. The reader may remember that it was in order to anticipate the detailed introduction of difficulties such as these that we laid down as a formal postulate the common acceptation and interpretation of language.

As regards then the "meaning" of a general term, it seems that three distinct views have been adopted.

(i) What appears to be decidedly the soundest and most workable account is to lay it down that the Connotation consists of those attributes which are generally recognized by careful speakers and thinkers as being implied by the name. There are, it is needless to say, difficulties in such an account. We may be asked, who the careful speakers are and how they are to be identified? We can only reply that the difficulty must be surmounted here as it is elsewhere, not only in sciences which deal with language directly, such as Grammar and Rhetoric, but wherever matters of taste and judgment are involved. We must take it for granted that we have somehow a capacity of selecting between the models to copy and those to avoid. Then again, in deciding what is signified by "implication" and how this is to be distinguished from mere suggestion, we find the line hard to trace. This question will turn up again presently, when we come to touch upon the historic aspect of some of our terms. At present it will suffice to say that, whatever may be the difficulty of decision in particular cases, we must insist that there is a real distinction between implication and suggestion; between being actually misinformed and merely misled; between a really incorrect use of a word, and one which is only awkward and misleading. We postulate that this distinction can be recognized and applied; and we are quite prepared to admit that, to the extent to which this postulate is departed from in current thought, to that same extent does Logic become a hypothetical or abstract science as distinguished from a concrete or applied one. On no other supposition does it seem possible to treat Logic scientifically and yet to make it of practical use.

We shall better see the drift of the definition here suggested

by comparing it with two other accounts, each of which is extreme in its own direction.

(ii) There is, for instance, a view which has been proposed in order to avoid the difficulties indicated just above. On this view a more objective,—that is, less conventional,—account of the matter is given, by regarding the connotation as comprising the sum-total of the attributes which are possessed in common by all the objects denoted by the name. Such a group of attributes is regarded as something assigned for us by nature, and therefore free from the caprices of language and the looseness of popular usage. The difficulties however to which such an account exposes us far outweigh any which it enables us to escape.

In the first place: such an arrangement inverts the natural order of precedence. We are supposed to recognize the set of attributes possessed in common by all the objects in a certain group or class. But what objects, and what group? The only available way of determining a class of things, especially when it is a class of potentially indefinite range such as those corresponding to most of our general names, is to begin with the attributes, assigning those in accordance with which the class is to be selected. That is, the connotation must theoretically come first, and serve to determine the denotation. We cannot rationally begin with a class taken, so to say, at random or by mere caprice, and then set about investigating what attributes its component members possess in common.

Again; we shall find ourselves led on further than we may wish to go. There are many recondite attributes, in various substances and organised beings, which have but recently become known: there are others which are now only known to one or two of the foremost discoverers. Are we to admit such properties as these, of which perhaps not one man in a million has ever heard, to be part of the connotation of the term? If so, we must either divorce the connotation from the 'meaning', or we must maintain that the meaning of the terms we employ comprises a number of attributes of which we never heard. Remember that, in determining the Connotation, we are, by general consent, determining the Definition; and that the definition has always been popularly regarded as comprising well-known attributes of the things defined. I should have

regarded the view in question as a reduction to absurdity if it could not claim the high support of Prof. Bain. He accepts the conclusion, going even to the length of admitting that the moment a chemist, say, has discovered some new property of a substance,—such as combustibility in a diamond,—anyone who predicates that property of a substance is merely uttering a 'verbal proposition', because he is repeating only what is now comprised in the connotation.

Again; on this view we are naturally driven by consistency to another conclusion which seems to verge on the absurd. We refer here to the case of *Proper names.* If the Connotation is understood to comprise all that is common to the whole class,—that is, every attribute which is present there whether the bulk of speakers have ever observed it or not,—it will naturally follow that the narrower the class the larger will be the number, not merely of common attributes, but also of those which are connotative or defining attributes; since these two groups have now been identified with each other. Carry out the process to the end of the scale, where the class becomes a minimum by consisting of but one individual, and what do we find? The number of attributes possessed by him may be considered infinite: therefore, on the principle of including all that exist in common throughout the class, we ought to admit that in this limiting case every attribute of the individual is a part of his connotation, that is, of the meaning of his name, though the overwhelming majority of them must, from the nature of the case, be known only to the individual himself. This one would have thought was a reduction to absurdity, but it has been adopted and defended by Jevons[1] with the distinct assertion that singular or proper names so far from being destitute of connotation "exceed all other terms in that kind of meaning". That one class of singular names may have a maximum of connotation is doubtless true, namely the class which we have already described as being built up of a number of significant general names. But even here the number of implied attributes is merely the finite total of what are given by the summation of the connoted attributes of the component group of names. The view in question entirely misapprehends

[1] Principles of Science, p. 27.

the nature of the Proper name. The express function of such a name when it is understood,—as it almost universally has been,—as an unmeaning mark imposed upon an individual for the purpose of distinguishing him, is to bar any such confusion, by drawing a clear distinction between names which do, and those which do not, imply attributes.

(iii) There is another view which takes the opposite extreme, and seeks to reduce the number of determining attributes to the utmost. This reduction may be effected in two slightly different ways, accordingly as we confine them to the smallest number which are sufficient to distinguish the group of objects in question from others, or to those which are sufficient to yield deductively all the attributes commonly reckoned to be included in the name. The full consideration of these views would lead us into a discussion of the nature of Definition, a subject which we must defer to a future chapter. At present it will be sufficient to remark that in each of these cases a very different design is being proposed from that which we have contemplated above; that, in fact, such writers substitute, for the direct enumeration of a number of attributes, certain devices for most conveniently distinguishing the objects in possession of those attributes. These may be very convenient practical substitutes, and in some kinds of definition for artificial purposes they may usefully be employed, but nothing except a change in our general point of view can ever make them any part of the *meaning* of the term.

We have several times incidentally introduced the distinction between 'implication' and mere 'suggestion', claiming that what falls under the former head is, and what falls under the latter need not be, part of the meaning of a term. It may be desirable to clear up this distinction a little more fully.

It must be frankly admitted that we shall find it no easy task to draw the line. By way of indicating whereabouts such a line may lie take the following example: I find in a parish register an entry of the burial of "John Thistlethwaite Barker, farrier": what sort of information can we extract from this bare designation? The answer would be that we know certainly that his business was to shoe horses: we know with a certainty which does not feel to us to be less that he was of the male sex; we feel sure that his father's name was Barker: and we

feel a presumption that he had some relation of the name of Thistlethwaite. I am inclined to think that the partition line between implication and suggestion must be drawn between the first two of these. The former seems to involve a matter of right and wrong, the others involve at most a violent presumption.

Much is to be learnt by a study of the way in which names are imposed, and of the way in which they are apt to acquire a meaning. That even proper names do sometimes acquire a meaning is certain, for we may hear it said of such and such a person of that name that "he is a regular Robinson", and so forth. When we come to look into the matter the fact is found to be that so inevitably do associations spring up upon persistent or even frequent repetition of any characteristic, and so readily do associations ripen into implications, that the question ought rather to be framed thus, Why do proper names *not* mean anything? How is it that they can continue to retain their character of being mere unmeaning marks? The only answer that can be offered is that under ordinary circumstances an individual presents himself under such a great variety of aspects that no one of these has time to get the upper hand. The many changes of mood and behaviour in the same man, and the many men going by the same name, hinder any such lengthened contact as will result in adhesion. But directly a man begins to present himself preponderatingly under some one aspect, or a family begins from one generation to another to display some fixed characteristic, we find the usual influences of association at work; and from association to implication the step is a short one. Thus we speak of a Nero, a Judas, of Cæsarism, of out-Heroding Herod, and so forth. The true logical proper name stands, in fact, upon a very insecure footing, and requires constant and peculiar influences to prevent it from falling into the rank of ordinary general names. But we must be none the less careful to retain in our minds an ideal of what such a name should be.

When we look into the matter historically we find the same facts forced upon our notice. It is however absolutely necessary that we should distinguish between the Connotation and the Etymology of any term. With the latter the logician has nothing to do. With him, for example, the name Brentford no

more implies a ford through the Brent than does Wednesday imply a portion of time which is somehow consecrated to, or named after, Woden. Whatever meaning such names once possessed has long since faded away, and in their current use they possess nothing but denotation.

It is no part of our business here to examine into the historic origin of proper names, but as the enquiry lies so close to our path a very brief notice must be devoted to this most interesting and suggestive topic. It appears then, so far back as we can trace our steps,—we must keep quite free from implying that this was really the first phase of human thought upon such matters,—that almost every name was once in some way descriptive; and that the disposition to impose names for the mere purpose of reference and identification is altogether a modern acquisition. To a primitive people this kind of arbitrary invention seems never to occur, and if it could occur to them it would probably seem a waste of good words. Even later, when one class of originally significant names has become unmeaning,—i.e. Christian names,—we may still observe that they were commonly helped out, in order to distinguish them better from each other, by descriptive attributes,—i.e. surnames;—and that it is only after a further lapse of time that these latter in turn take their place amongst the true proper names of the logician. There was a time when 'Isaac Thatcher' consisted of a proper name differentiated by a descriptive common name, just as there was a still earlier time when 'Isaac' itself, having a distinct and deliberate significance, might almost be termed a descriptive common name. At the present time both elements stand on the same footing as proper names. But the experience of every school, workshop, and regiment, shows how naturally we select some descriptive or connotative term to aid in determining an individual, especially when his proper name by itself is not sufficient for the purpose of identification.

If it be asked, where then are we to look for instances of names which have from their first imposition rigidly satisfied the logician's requirement of being mere arbitrary marks imposed upon an object? We can only reply that such are hardly to be found except where civilized and mature persons are concerned with numbers of objects which it is important for them

to distinguish, and to which they have frequent occasion to refer. Truly typical instances are to be found in the names of race-horses, ships, and, for the most part, in those of newly built houses and streets. To these might be added the numbers by which convicts are distinguished, provided this is how they are commonly referred to in the prison, and that no indication of their order of conviction is thus conveyed. Various other analogous instances might be found which should satisfy the logician's requirements.

IV. *Concrete and Abstract Terms:* The next distinction which we have to notice amongst our terms is that between concrete and abstract. As these terms are commonly defined in the text books, the notion seems to be suggested that they are *absolute* designations, in the sense that if any term be proposed to us by itself we ought to be able at once to refer it to one or other of these classes. We shall, however, see reason for considering them to be *relative*, in the sense that we can at most say of two of them, when proposed together, that they stand to each other in the relation of concrete and abstract.

The account commonly given of the distinction is that a concrete term denotes a thing, whilst an abstract term denotes an attribute of a thing. But, as we have already seen when discussing the preliminary postulates of Logic, we soon find ourselves launched into a sea of perplexity when we ask what a 'thing' is. So long as we are left to choose our own ground in the selection of our examples we can of course mark the distinction sharply enough. We may say, for instance, that a horse is concrete and its colour is abstract, and so on. But material objects of this well-defined character form but a small part of our stock of words in common use. A horse is doubtless a thing or object to almost all sentient and percipient creatures; but can we say the same of what are to us such concrete entities as a Parliamentary election, or a Boat race? A good deal of analysis and synthesis, of abstraction and limitation, has to be gone through before these objects are recognizable as individualities even by human faculties; and the most intelligent of animals would fail to make anything of them. By successive processes of this kind we may obtain higher and higher abstractions, each of which may be considered, by

comparison with those from which it was derived, as being abstract; and, in turn, when compared with those derived from it may be considered concrete. 'Party spirit' might be reckoned an abstract quality of a political party;—which is itself by no means so concrete an entity as one of the persons composing that party. The virulence of that party spirit may again be reckoned as an attribute derived from the spirit itself, and so on. The fact is that hardly any object, as objects are distinguished by us, can be selected, which is not to some extent a product of our powers of abstraction, and the more or less of this faculty called into play in any particular case hardly warrants us in labelling the instances respectively with such distinct designations.

Regarded as a purely logical distinction, therefore, the one now before us does not seem to be of great value. There are however certain aspects of the question,—grammatical and psychological rather than strictly logical,—which are best noticed in the present connection.

(1) As regards the judgment of the grammarians on this matter, I apprehend that they make the distinction turn mainly upon the derivation of the terms. We very often find pairs of correlative terms of which one is primary and the other derivative, the former referring directly to a group of objects and the latter to some quality which those objects possess in common. In such cases the distinction between concrete and abstract is of course clearly enough marked: for instance, human, humanity; friendly, friendliness; white, whiteness, and so on. The fact that many logicians,—Mill, for instance, along with others,—select their examples from such as exhibit also this grammatical characteristic, makes it difficult to feel certain whether they really regard the distinction as going down deeper than can be accounted for by mere etymology.

(2) A psychological and philological question of some interest is involved in the comparative priority, in the development of human thought and speech, of concrete and abstract terms. The question is a very difficult one. In its reference to the growth of the individual mind, namely that of the child, it is complicated by the fact that children grow up under the all-powerful influences of a language which is in a stage of development far ahead of their own. The constant use of abstract

names within their hearing must greatly facilitate the process of grasping the characteristics which these denote. Broadly speaking there can be little doubt that the recognition of concrete objects is the earlier. It is, for instance, incredible that 'man' should not be understood, that is, be used with some degree of correctness and appreciation, before 'humanity', and even 'black' before 'blackness'. But to this rule there must be many exceptions. Bearing in mind what exceedingly artificial mental creations many of our concrete notions are, it is tolerably certain that they would be acquired long after some of our simpler abstractions. Compare for instance the concrete 'democrat' with the abstract 'greenness'. This of course raises a slightly different question, and in any case does not concern the growth of ideas in the child. But it seems to me that whole classes of terms which refer to mental qualities must be first and most completely grasped under their abstract conception. For instance, 'envy', which must be considered the abstract corresponding to 'envious', would surely be the easier of the two to acquire; partly because we are helped out here by a direct appeal to our own feelings. But, as already stated, the distinction between the two classes of terms seems so slender, and in many cases so artificial that it is hardly worth the trouble of insisting further on this point.

As regards the comparative priority of these conceptions among mankind generally, or in any particular race of men, in which cases alone we can consider the question as being freed from the constraining influence of an already highly developed speech, two methods may be resorted to. We may treat the problem deductively, on psychological grounds; or we may endeavour to decide it à posteriori from conclusions drawn from the study of language. Anything beyond a passing reference to these considerations would be out of place here. It may just be remarked that so far as psychology can guide us there can be little doubt that if the clear apprehension of one precedes that of the other, it is the concrete which takes the lead. But many exceptions would probably have to be made, of the kind indicated above, for the simpler abstractions are often far easier of attainment than many of the more complex and artificially selected concrete realities. And in any case,—here as in all other kinds of mental progress,—there is a continual process of

mutual aid and support. Any advance in the one direction implies somewhat of an advance in the other; indeed none but a very rudimentary appreciation of the one quality could be secured without some appeal to the other.

As regards the conclusions to be drawn from the results of Comparative Philology, the main defect is that such enquiries can carry us such a very short way back in the history of mankind. Max Müller, and some other investigators, consider indeed that the question is answered by their finding that the primitive roots of speech represent abstract qualities. For instance, at the base of all the terms indicating cave, vault of heaven, and so forth, they claim to find the abstract root 'hollowness' rather than any concrete conception indicating 'a hole'. It would be absurd to offer any criticism of a theory held by such authorities, by a mere digression in a work on Logic, so I will merely repeat that such enquiries reach a very little way back for our purposes, since we do not thus get within ages of the real origin of speech.

V. *Positive and Negative Terms.* The next distinction we have to consider is that between positive and negative terms, as they are often called. We shall find it best to approach the distinction, not directly, but by first considering what is the general relation of which this antithesis marks a particular case.

(1) We begin then with terms which stand to each other in the mutual relation of contradiction. It is obvious that terms may often be found which go together in pairs, in the sense of their being mutually exclusive and collectively exhaustive in their denotation. That is, neither of the names is applicable to anything to which the other can be applied, but between them they cover the whole field of application whatever that may be. There are two distinct ways in which this relation may find expression, which may be called respectively the material and the formal contradiction.

(i) Material contradictories are those which are not constructed for the express purpose of indicating their mutual relation. The contradiction can be detected in fact, but is not implied in the names. In the formal contradiction it is enough to understand the meaning of one term to understand that of the other; in the material contradiction each term demands

separate interpretation. These latter are mostly to be found in cases where each of the two classes of things stands, so to say, upon a footing of equal right; that is, where each group of things presents itself in so many, and in such important relations, that it has acquired an independent name of its own. Thus, in popular phraseology, British and Foreign; and, in legal phraseology, British and Alien, may fairly be regarded as contradictories. Within their range of appropriate application,—which in the latter case includes persons only, and in the former case is extended to produce of most kinds,—the two pairs of terms fulfil tolerably well the conditions of mutual exclusion and collective exhaustion. The requirements for the occurrence of names of this description are of course rather peculiar, and they are therefore by no means frequent. It is presupposed that each group of things has some familiarly recognized attributes in common, of a positive kind; other than the mere negation of the attributes which bind together the members of the correlative class and furnish the connotation of its name. In other words, whatever is wanted, in order that a class should be commonly recognized as such, must be present independently in each of the pair.

Here a difficulty meets us. No pairs of terms can be found which shall fully satisfy the condition of just combining to cover the whole range of existing objects. In popular convention there are invariably limits presupposed, wide or narrow as the case may be, within which the range of application is supposed to be confined. Nature is far too extensive, and the objects which constitute it are far too heterogeneous, for us to be able as it were to sever the whole field across at any point, and expect to find each of these portions pervaded throughout by common attributes of familiar appreciation. Accordingly all the contradictory pairs of terms of this description, which can be selected from popular language, are found to be limited in their application by well understood restrictions; this limitation constituting what has been already described under the name of the Universe of Discourse. Sometimes this range is very wide. Thus, male and female, as also material and spiritual, cover an enormous area. As the Universe becomes more restricted, the pairs of contradictories recognized in common language become more frequent; but *pari passu* the propriety of speaking of

'contradiction' when we know that the total range of the objects with which we are concerning ourselves is becoming narrow, is rendered more questionable.

(ii) In order to avoid this difficulty the logicians have adopted a hint from popular speech, which they have developed far beyond anything which popular usage would consent to accept. This is the plan of marking the contradictory of an assigned class *formally*, that is, by some kind of negative particle. The plan of thus constructing an artificial class enables us to put into it what we please, and to make it as wide as we like, without any necessity for its being pervaded throughout by other common attributes. Popular speech has plenty of well-recognized particles of this sort at command; for instance, *in*human, *dis*agreeable, *un*likely, and so forth. Each of these derivative names is, in perfect strictness, intended to denote all appropriate objects to which the name from which it is derived does *not* apply.

A moment's consideration of such popular names as these will remind us, if we need reminding, how entirely our common speech has grown up under the stimulus and the direction of common needs. Accordingly such names will not quite answer the purposes of science, and the logician in developing his scheme finds himself forced to depart from common conventions in two respects. In the first place he insists upon extending the denotation of his contradictory term indefinitely, instead of strictly limiting it to what is considered relevant matter. In popular speech 'inhuman' is always confined to the same general sort of things to which the term 'human' could reasonably be applied. The logician knows nothing of reasonableness in this respect, and insists upon extending his negative as widely as he pleases; to anything in fact which is not 'human'. Again; popular thought, avoiding always sharp distinctions and broad generalizations, gets rather into the habit of separating any two such contradictories by a sort of middle zone or neutral ground. The frequent use of a word has a tendency to fix an average value for it, and this average naturally lies at some distance from the real line of partition. Hence the denotations of two such terms as 'kind' and 'unkind', tend, so to say, to shrink somewhat apart; and, instead of covering the whole ground, they generally leave an intermediate space between them which is appropriately occupied by neither.

(2) Accordingly, to make his usage clear, the logician introduces an artificial technical term for the purpose of excluding any such neutral ground. He prefixes the particle 'not', or 'non' to his terms, with the object of unambiguously covering the whole remaining ground. Thus 'human' and 'not-human' are intended to act as a more unreserved pair of contradictories than 'human' and 'inhuman'.

It will be observed that though the denotation of these logical or formal contradictories does not in any way differ from that of a contradictory assigned by an independent name of its own, viz. a material contradictory, yet the connotation is decidedly different. The connotation of a term of the type 'not-X' is entirely confined to denying that of X, whatever this may be. This denial of course applies to the group of attributes implied by X taken as a whole; that is, it is secured if any portion of them can be denied, though the others remain present. A thing is 'not-human' if any one of the constituent attributes of 'human' is missing from it. On the other hand, any term which plays the part of a material contradictory to another must have an independent connotation of its own. Its meaning must consist of some positive attributes selected from those which the objects in the group possess in common.

(3) In many cases the inconvenience or impropriety of dividing any large class dichotomously, that is, into two subdivisions as above, and two only, becomes very marked. The magnitude and intricacy of nature are too extreme for it to lend itself readily to such a simple arrangement as this. What we may find, after separating off one class, is that the remainder, instead of adhering naturally together as a whole, splits up into several distinct classes, each of these being held together by its own constituent group of connotative attributes. When an aggregate class is thus divided into a number of subordinate mutually exclusive classes, the technical expression formerly in use for designating these several classes was *disparate*.

When disparate classes of this kind can be arranged in some kind of progression, in the sense of possessing more or less of some quality; so that we can select two classes and say that they are more remote from each other than any other two are, the technical name applied to such relation is *contrariety*. Thus 'revolutionary' and 'supporter of the divine right of

kings' might be regarded as contrary classes; these being the two such classes which stand at the utmost distance from each other in respect of the opinions in question. Black and white might equally be regarded as representing a pair of contraries in respect of colour.

As regards the terms Positive and Negative, as applied to terms, there is no very clearly recognized logical doctrine, as the expressions are popular rather than technical. Perhaps the best account we can give is to say that of two formal contradictories,—such, that is, as popular speech is in the habit of using,—the one which bears the negative particle is the Negative, and that any term not so furnished is to be ranked as positive. In the case of the technical contradictories of the logician there would be no doubt that we must so interpret the terms. In other words, terms may be regarded as positive or negative according as they indicate the presence or absence of an attribute. Any one, however, who bears in mind how wide is the interpretation we are obliged to give to what we call an attribute, will realize that it is no easy matter to say in every case whether any given state of things is best described as indicating the presence or the absence of attributes.

CHAPTER VIII.

PREDICATION AND PROPOSITIONS.

I. PROPOSITIONS, as a general rule, are analyzable i subject, predicate, and copula. The recognition of these three distinct, or distinguishable, elements is a commonplace of Grammar and of Logic alike, and is too familiarly adopted to stand in need of mere explanation or justification here. What concerns us at the present stage is rather to enquire into the convenience of such a division, and into the principal logical consequences which follow from its acceptance.

A few words of historical reminder ought to be prefaced. Whatever sources of information we appeal to; whether to comparatively *à priori* psychological considerations, to the data furnished by the earliest written records, to the speech of the more backward peoples of the present day, or to the innumerable indications furnished by surviving elements in the more cultivated speeches; it becomes equally evident that the starting-point of the logician here is by no means the only available one. There can be no doubt whatever that the familiar three-fold fissure of the proposition is very far from being found to be universal, when our enquiry is widely extended. So far indeed from three elements being found to be necessarily distinguished, it is maintained by some enquirers that the natural and primitive form of speech involves them inseparably together, so that the natural unit of speech is a whole sentence. A stock of such expressions, standing for the sentences most in use for the rude wants of an early people, would thus constitute their language.

But however this may be, there is certainly no need to go back to the remote past in order to find examples of propositions in which the three elements are not distinguished from each

other. The logician may say that they are involved there, but what he really means is that, by altering the structure of the proposition, he may throw it into a form in which these elements are separately expressed. For instance, there are the so-called existential propositions,—such as, There is a devil,—about which we shall have something more to say presently. There are the impersonal propositions,—such as, It rains, it is sultry. Nay, there are more compendious forms than these, for many interjections ought distinctly to be ranked among propositions, if we attend only to their obvious significance. Some of our interjections are of course mere vents for feeling, or intimations of danger or pain too vague to deserve to be considered as communications of definite ideas. Persons who have reached the stage in which they habitually make use of articulate speech, will naturally resort to it in cases where mere outcry would suffice. There are, however, many interjections which we must insist upon regarding as to all intents and purposes propositions. Thus 'Thief' and 'Fire' are exactly equivalent to assertions that such agencies are then and there at work on the spot in question. He who raised these cries without ground would be universally considered to be coming much closer to a lie than he who merely uttered a scream when he was not hurt. It would be said that he had raised a *false* cry. We may call such interjections, if we please, condensed or abbreviated propositions; but if we do so speak we must remember that this is merely true in the sense that we may if we please expand them into the standard form. There is not the slightest reason to suppose that they ever were expressed in that fuller form, and subsequently underwent contraction.

Fascinating as such enquiries are for the philologist, the logician's proper task is a much narrower one. He has not to enquire whether the three-fold propositional form is the spontaneous or primitive form, but merely whether it is a possible and convenient form; and on this point there can be no difference of opinion whatever. As regards the possibility of the general adoption of this form,—that is, of articulately expressing the subject, predicate, and copula,—this seems to follow from the very nature of thought. For thought, so far as Logic is concerned, involves in every case a process of synthesis and analysis, of framing attributes and joining or disjoining them.

Even what are commonly called existential propositions, as we shall presently see, fall under this head, for the existence, in every case where it need be taken into account, can be regarded as being of the nature of an attribute, or as involving analysis and synthesis. And the merest interjections, if they are consciously intended to convey a knowledge of our state of feeling at a certain time and place, as distinguished from other times and places, may be similarly interpreted. Now any process of synthesis or analysis will furnish at least three elements, viz. the two things which are joined or disjoined, and the act or result of thus joining or disjoining them. And this is enough, as we shall soon see more clearly, to furnish us conveniently with a subject, predicate, and copula.

The practical conveniences of this analytical mode of expressing our propositions are very great. As, however, some of the grounds of this convenience may not be familiar to those who have not given attention to the nature and employment of language, it will be worth while to expend a short time in examining them somewhat in detail.

(1) For one thing, on merely arithmetical grounds, there is an immense economy in the number of symbols required in order to express our thoughts. The reason of this is exactly the same as that which recommends the employment of separate letters to build up our words, instead of representing the words as wholes by single symbols. Wherever we are concerned with a number of various wholes which consist of different combinations of the same set of elements, or which can be so analyzed as to appear as such, then we generally find it best not to use separate symbols for those wholes, but to symbolize the separate elements instead. Of course we shall also need theoretically some kind of symbol to indicate the nature of the combining act itself; but when, as in most cases, we are only concerned with one or two such kinds of combination the extra complexity thus introduced is very slight. The economy which results from this method of treatment has already been indicated in connection with the doctrine of Terms. It finds its full significance in the Symbolic Logic. We need only point out here the obvious fact that five subjects and five predicates may be put together in twenty-five different combinations. Accordingly, if we have two kinds of combination to take into account, it is

plain that twelve distinct symbols will do the work for which we should require fifty if we insisted upon having a distinct expression for every sentence as a whole. In fact a language which would break up into nothing smaller than sentences would be as far behind ordinary existent languages, as one which adhered to hieroglyphics would be behind those which employed an alphabet.

In the last chapter, when speaking about *terms*, we described the arrangement which might be called one of 'fixed subjects with variable attributes', and showed that though it could not be considered strictly accurate in matter of fact, it afforded great conveniences for treatment. The common propositional form fits in admirably with this arrangement. Any simple example will serve as an illustration. Two such statements as 'The apple is green', and 'The apple is red', might have to be expressed by two entirely distinct symbols, if we employed a sentence-language instead of a word-language. And if we had regard solely to what is present to the senses at the time, there would seem to be something in favour of such a course; for in a hurried glance the two presentations to which we thus give expression have very little indeed in common, consisting, as they do, of not much more than a green and red surface respectively. The common basis of fixed attributes which serves to constitute the subject is, for the most part, not present to sense at the time but has to be supplied by the mind. This common basis consists of such characteristics as the similar shape, smell, taste; the form, height, and foliage of the trees on which they grow, and so forth. It is obvious how much this process, of retaining in mind what is not present to sense at the time, is aided by having one constant symbol to stand for the whole group; in other words, by throwing our sentences into the subject and predicate form.

(2) Again; it must be remembered that what Language has to do is not merely to serve the purpose of conveying information as to which many persons are already certain; but it must also aid us in the actual process of acquiring new information. We want such aid all through; from the first glimmerings of suggestion to the ultimate state of certainty. A scheme of sentence-symbols would not only be incompatible with any but a very narrow range of thought and expression,

but would also seem to demand that the same degree of certainty should prevail over all this range. This might fit in with the scanty needs of primitive people, and with the black and white character of what they do and do not consider themselves to know, but would be quite out of keeping with the intricate web of knowledge, shading through one degree after another of doubt down to utter ignorance, which goes to form our actual acquirements in almost every department of thought. Such a state of things can only be grappled with by starting with a 'subject', throwing into this subject all or part of what we feel certain about, and then tentatively attaching 'predicates' to it. In this way we can make our path sure, step by step, so far as certainty can be acquired; and if we cannot attain certainty then we can with equal convenience give expression to our doubt. Such is the suitable verbal framework corresponding to our actual position in most of the enquiries into which we have to make our way, and it is not easy to see how else the process of evolution of thought in beings possessed of minds such as ours could possibly take place.

II. As already remarked, most propositions in their customary form already indicate the process of analysis and synthesis, and all may be made to do so by a certain transformation. The reader will be better able to understand what extent of transformation is required if we examine in turn a few classes of cases selected in order to display the gradual increase of complexity and artificiality involved. This will also serve conveniently as an introduction to the next enquiry before us: What is the logical distinction between the subject and the predicate? It will be understood that we do not here propose to make an exhaustive classification of the various kinds of predication, but rather to offer some indication of the range and significance of logical predication.

(1) Perhaps the simplest and most familiar kind of predication is that in which we take a *substance*,—in the ordinary sense of the term,—and connect an attribute with it. For instance; the stone is heavy, the fruit is ripe, the lion is dead. The reader must be reminded that even in such cases as these the unity of the substance is in part the result of our own process of synthesis, though this process is so obviously and

naturally performed that the unity must have been recognized by every one from the earliest stages of observation and thought. The mode of expression in accordance with which we thus speak of the same substance having,—at different times of course,—different and conflicting attributes, has been strongly objected to, on the ground that any alteration of an attribute is an alteration of the substance in which it is supposed to inhere. The fruit, for example, is clearly not 'the same thing' when unripe and when ripe. I cannot but think that this objection is hypercritical, and that it rests in part upon a misapprehension of the true significance of names. Of course if a name were supposed to indicate every attribute which the thing possesses, it would be a contradiction in terms to retain the name after any attribute had undergone a change. But the whole doctrine of the distinction between what is essential and what is accidental in a name, is intended to guard against this. The connotation of the name is expressly confined to a selection only of the attributes, and therefore so far as these are concerned there can be no formal impropriety in using the same name under different circumstances.

The case is certainly slightly altered when we are dealing with more fundamental or essential attributes. When I say, the stone is heavy, or possesses weight, it may fairly be objected that a stone without weight would no longer be regarded as a stone at all. If language were constructed with ideal precision, and if there were no objection to the indefinite multiplication of terms, it might be desirable to amend our expressions in this respect. As things are, there does not seem to be any serious inconvenience involved.

(2) The next step in advance from this may be considered to be that in which,—still dealing with what would commonly be regarded as substances,—the predicate which we attach to our subject has become highly complex, artificial, or remote in time or place. Compare, for instance, the propositions: the fruit is ripe, and, the fruit is deadly; or, the picture is square, and, the picture is an heirloom, and so forth. We are using throughout the same word "*is*" for predication, and in each case we are considered to be attaching an attribute to a subject. But it is easy to see that the conception of inherence of the attribute in the subject has been rather widely stretched here.

A remote consequence, or one of a considerable degree of complexity, has been grasped up by the mind into a unity, and is viewed as though it were in present connection with the subject.

As regards the mere complexity of the attribute in such cases as these we need hardly repeat what has been said already. The apparent degree of complexity very much depends upon the trouble we should have to take in order to analyze the thing in question. Any thing will admit of analysis up to almost any point, when we choose to set about the process, and it would be very hard to say in what sense, strictly speaking, one predicate could be regarded as being objectively simpler or more complex than another. What we really mean in the case before us is that certain attributes are such as strike everyone at an early stage of experience, whereas others demand long consideration, or depend upon the exercise of specially cultivated faculties, or are the product of a complex or highly developed state of society. Thus the squareness of the picture must, in its elements at least, strike every percipient being, whether or not he has a name for it; but the conception of an heirloom is not easy except in a settled state of society, and is the result of some cultivation and reflection even there.

As regards the present attribution of what is to all intents and purposes nothing but a future consequence, a few words may be conveniently added here. It has been much debated whether the only admissible logical verb, the copula, ought to be confined to the present tense. On this point the general judgment of logicians has been in the affirmative; and doubtless correctly, in so far as this is a recognition of the fact that the essential characteristic in all kinds of logical predication is of one and the same kind. I should however myself be inclined to extend this admission further even than most logicians do, by including also under the same head of predication certain propositions,—i.e. hypotheticals,—which are generally classed apart. But of this more will have to be said in a future chapter. Confining ourselves for the present to the simpler cases in which agreement of treatment is tolerably complete, we may easily see that it is advisable to take very little heed to the distinction between past, present, and future, so far as our predications are concerned; that is, it is best, for formal treatment, to keep the copula in the present tense.

The fact is that in every case of predication we have to view, as a present synthesis, elements which are actually scattered about in time, and therefore cannot be present simultaneously to the senses. Even in the case of a substance, with what are commonly regarded as permanent attributes, such as the weight, shape, smell, and taste of the apple, this holds true. It would require a good deal of careful manipulation to succeed in realizing them all simultaneously, and as a rule we generally have but one or two of the qualities present in sensation at the same time. We are forced, as a rule, to experience them successively, but what gives us the notion of simultaneity is probably the fact that we can reverse or vary at will the order in which they are experienced, and call up any of them again whenever we are so disposed.

Being thus accustomed to connect together, with a subject and predicate, qualities which to us individually, at any given time, are not simultaneous, it is only a step on in the same direction to connect together by a similar formula properties which can never be simultaneous to any one. Take, for instance, the deadliness of the fruit. By the time the fruit has proved its poisonous qualities, it has really ceased to be a fruit, for all its previous sensible attributes have undergone a complete change. It is of course open to us to save the present nature of the poisonous attribute here by maintaining that the chemical constituents of the substance which render it poisonous were always present along with the original shape, colour, and so forth. True; but those who first gave it the name probably thought nothing of this when they began to use the present tense of the copula verb in such cases. To them, as indeed to us at the present day, the whole significance of the predicate lies in a future result; and in a result, remember, which is entirely conditional upon the rare occurrence of the fruit being swallowed. Similarly, when we say of the picture that it is an heirloom. The predicate sums up and signifies a whole train of history in the past, and if we had no single predicate such as this to make use of we should find ourselves obliged to enter into a somewhat long description, in which we should be employing throughout verbs in the past tense. In all such cases it is the function of logical predication to gather up into a unity attributes grounded on events which may only occur in the past or in the future.

(3) Now consider the way in which we exercise this power of mental synthesis in relation to those complex subjects which are in great part our own production. These correspond to what Locke called "mixed modes", and which he contrasted so sharply with substances. He greatly exaggerated their arbitrary character when he maintained that, whereas nature sets before us types of substances so clearly that we cannot but recognize them, we find ourselves at liberty to exercise our own arbitrary choice about the mixed modes. If we are to think and to use names to any purpose we must do so in accordance with the actual conditions of life. So regarded, many of these so-called artificial entities give us little more option than we feel when confronted with a real substance. 'Murder', for instance, is commoner than many metals, so far as our ordinary experience is concerned, and is quite as definite and determinate as many more material conceptions. It presents itself to human notice; it had already got a name long before any one of us was born; and if there were any question as to the correct use of the name we might appeal to the experience of known instances in order to decide our usage.

None the less, however, instances of this kind, taken as a class, do certainly represent a decided advance beyond the simple substances and persistent attributes with which we supposed ourselves to start. They remind us of the continual synthetic process which is required in order to describe the ever increasing complexity which the march of evolution entails. We of the present time recognize, as subjects of our propositions, such entities as 'The Christian Dispensation', or 'The political status of woman'; and to these subjects we may require to apply such predicates as 'suited to the needs of Western civilization', or 'much discussed in certain circles at the present day'. As we have already had occasion to point out, the reader must not confound verbal or formal multiplicity with real intricacy. The most complicated subject may be indicated by a very simple term, and probably will come to be so indicated if we have very frequent occasion to refer to it. A much compounded term is generally the symbol, not merely of a complicated object but of one which is either but rarely referred to, or which has come into recognition too lately to have acquired a more terse and familiar appellation.

(4) In the preceding cases the group of attributes which constituted the subject,—whether or not there was a material substance underlying it,—was sufficiently large and stable for us to be easily able to conceive any assigned attribute as being separated from it or re-attached to it, without seriously compromising the integrity of the group. Its mass, so to say, was sufficiently great for it to be able to bear such slight losses and gains without its centre of gravity being seriously shifted. And such a state of things might almost seem to be a condition requisite to the possibility of a sentence being cast into the subject and predicate form.

It may, however, happen that one of the above-mentioned synthetic groups is composed of very few attributes, so that the detachment of one of them to form a predicate, whilst the remainder are left to constitute the subject, will break rather seriously into the integrity of the latter. Take an example. What do we mean by 'the weather', in common discourse? Presumably nothing more than the general combined condition of the heat, moisture, and wind, experienced out of doors. Hence to predicate any one of these three qualities of 'the weather' comes very near to counting it twice over, since the subject without it would scarcely be able to claim the name. Still more if we were to go on to predicate all three qualities at once, by saying that the weather is cold, wet and gusty; in which case the subject seems reduced to a sort of phantom or fiction[1].

The fact is that assertions of this kind fall more appropriately into the form of *existential* propositions, and it seems to be owing merely to our familiarity with the subject-and-predicate form of statement, and to most of our language being cast in that mould, that we ever apply it to such extreme cases as this. Indeed we should quite as naturally say, It is cold, wet and gusty, as introduce a sort of fictitious subject to which these

[1] "There was a severe frost in the metropolis, and this, coupled with bitter winds from the East and North-east, rendered the weather extremely cold" (*Times*, 19. 1. 88). Contrast this with a precisely analogous verbal form where we are dealing with some material object and its group of attributes: e.g. 'There were many passengers inside, and this coupled with the luggage on the roof, rendered the coach extremely heavy'. It seems obvious that the 'subject and predicate' form has much better justification in the latter case than in the former.

attributes are to be attached. The former is intrinsically the more appropriate; for in it we simply predicate the presence of three attributes or occurrences, whereas in the latter we feign a subject which really consists of scarcely anything else than the very three things which are assigned to it.

As this seems the most appropriate place, a few words more may be added as to the nature of these impersonal propositions. It seems to me that the most convenient way of regarding them is to consider them as constituting the extreme opposite of those which predicate comparatively accidental and trifling properties of some fixed substance. It is to these latter that the subject and predicate form seems the most appropriate. On the other hand, when we are considering some group of events with nothing in the nature of a fixed substance underlying them, it would seem that the most natural form would be some single expression or term indicative of that group, with of course some kind of inflection which should imply the fact that it was actually occurring at the time in question or at some other time. It is no doubt an easy task to say what ought to be the convention adopted when we know what actually is adopted, but having made this admission we might suggest that the appropriate form would be one closely analogous to a mere term. What is meant, for instance, by the term '*thaw*'? A certain group of events, the melting of ice and snow and the softening of what was hardened before. The utterance of this term does not tell us that such an occurrence is taking place at this or any other particular time, though it raises a strong presumption that it is known to occur some times. If we want to express the fact that the phenomenon in question is occurring at this particular time we should naturally adopt some closely analogous form, and this we find in the impersonal proposition, "It thaws". Whether or not this at all corresponds to the origin of such expressions,—whether, that is, the term or the proposition was actually prior,—there can be little doubt that this form is both convenient and suitable. In other words, just as certain natural occurrences are appropriately indicated by the subject and predicate form, others are equally appropriately indicated by the simple impersonal form.

One caution must be insisted on here, which is often needed when we are engaged in defining current forms of speech, and

which will recur again when we come to deal with Hypotheticals.

The reader must remember that to speak of one kind of occurrence as being appropriate to the predicative form and another to the impersonal does not at all imply that they are exclusively so employed. As a mere matter of actual usage, there are many cases in which we can employ either form, or in which different forms are adopted by different nations; and as a matter of possible usage it is hard to invent a case in which we could not adopt at will whichever form we please.

For instance, that convenient abstraction 'the weather' stands as supposititious subject to many a predicate which has little right to anything of the kind. No difference of fact can be detected between the meaning of the expressions, 'It blows hard' and 'The weather is boisterous'. Still more instructive is the comparison between the German and the English languages. The former, as Prof. Sigwart has pointed out[1], has a special partiality for the impersonal form and uses it in a multitude of cases in which we should prefer the other form. For instance, if there is any one thing which to us would seem to have a right to stand as a subject it is our own personality; and on the strength of English usage we could argue that no other form was reasonable than 'I am cold', when I want to express the fact of experiencing that sensation. But to the German it seems quite as appropriate to regard the sensation as an event or occurrence by itself, so to say, and to put it 'Es friert mich'. Again we make 'the ghost' an object and state that it 'haunts the house': the German prefers the impersonal phrase 'Es spukt im Hause'; and so on.

(5) There still remains one more case, which must be reserved however for discussion in a separate chapter, and is only noticed here because its entire omission might mislead. In all the preceding cases, in which the predicative form was habitually or occasionally used, we found that there were two elements connected together in the mind. That one which constituted the subject was in almost every case the most important or substantial one, but the same form was still occasionally

[1] See his very interesting and instructive Essay *Die Impersonalien*, 1888.

employed even when the subject had dwindled down into insignificant proportions; and it could indeed be employed, on occasion, when that element had disappeared as a separate one. Suppose now that we have two elements which are obviously of comparatively equal importance and solidity, so to say; can we connect these together in the same way, by predication, as we connect a substance and its attributes? Suppose that they are two events,—simultaneous or successive, proximate or remote:—when we want to make a synthesis of them by mentally connecting them in the way in which we believe them to be physically connected in nature, is there anything to prevent us from employing the predicative form? If one of them be symbolized by X and the other by Y, may we express the relation in any such form as 'X is Y'? This question is merely raised here, because the context seems inevitably to suggest it. The subject will be discussed under the head of Hypothetical propositions.

III. The foregoing remarks will have served in some part to furnish an answer to our next enquiry, namely, What is the nature of the distinction between the subject and the predicate of a proposition: how are we to determine which of the two is the subject in any given case? Various answers are given as to the nature and characteristics of this distinction; but we need hardly remind the reader that all which here concerns us is the logician's account of the matter, not that which the grammarian may find it convenient to adopt.

One account of the distinction is to the effect that the subject is that of which something is affirmed, and the predicate is that which is affirmed of it. This is substantially the account which Mill gives, but of course without professing that it amounts to a real definition. Indeed, if it aimed at being a definition it would be something of a circular one, for we should find it difficult to understand what is meant by affirmation unless we had already distinguished between the subject and predicate; and in any case we should find it of little use to appeal to such a test in any doubtful cases. Hamilton gives an account which, with due allowance for his notional phraseology, seems to me to be somewhat nearer what we want. He holds that the subject is that which we think of as the determined, and the predicate as the determining notion.

For consider what actually passes through the mind when we frame a sentence with its two constituent elements, of subject and predicate, as above indicated. Certain exceptions apart (to be considered presently), one of these notions or terms is readily seen to occupy the prominent place: it stands first in the natural order of thought, so far as that proposition is concerned, whether or not it had been the first to enter the mind or to be uttered in words: it is that which is modified by the conjunction or disjunction of the other. For instance, when we say, The stone is heavy, or Heavy is the stone, in each case alike it is the stone which stands foremost in the mind. When the proposition is deliberately thought over, what we realize first are such items as the shape, colour, size, and so forth. We start with a certain number of such individualizing characteristics, more or less as the case may be, and to this group of attributes we mentally add on, or recognize the presence of, the further attribute of weight. We do not try to take them in the reverse order, by thinking first of the weight and then adding on the rest of the group to this. And so in the great majority of propositions, especially in those in which a substance or something substantial is present to claim the place of a subject.

It may serve to illustrate the point in question if we revert for a moment to the case of *terms*. The comparison between the formation of complex terms and the framing of propositions has occupied our attention already; and it has been pointed out that every such term contains in itself the ready materials for a number of propositions, and that what may be called the outcome or result of the assertion of a proposition may remain behind as a permanent deposit in the form of a term. But at present what we have to call attention to is a point of distinction between them. It appears to me then that the most prominent characteristic of predication is the deliberate holding out before the mind,—whether for the purpose of junction or disjunction,—of the component parts of the proposition as separate elements, whereas in the case of complex *terms* the two elements are united together and thought of merely in combination. Now when they are thus held up separately some kind of order or precedency is possible, and indeed inevitable from the constitution of the mind, and this at once gives an opening to the distinction between subject and predicate.

Thus in 'The horse is black', we deliberately distinguish the horse from the blackness, but intimate that the attribute is to be joined on to the subject. The process, at the moment, is the same whether we supposed ourselves to have the proposition already before us or to be in the act of making it for the first time. At the instant, we are keeping mentally apart two elements which we say actually adhere together. Now contrast with this the *term*, 'The black horse', where we have exactly the same two elements before us. Here we think only of the result: of the fact that the two are to be found together. It was previously pointed out, when we were discussing the nature of these complex terms, that we could hardly say that there was any fixed order in which their component parts successively took their place in the mind. Language, of course, forces us to adopt a verbal sequence, but the mind takes no account of any such priority: the elements seem to take up their positions unconsciously to us, and on their first appearance to show themselves as a group. When we enumerate the component elements of such a complex term we feel that the order is,—for any but poetical or rhetorical purposes,—absolutely insignificant.

In other words, though the term and the proposition both agree in involving a synthesis, the former does so only as a result, the latter as a process. And hence the fact that the latter inevitably gives rise to the distinction between subject and predicate in respect of the elements involved in it, whilst the former does not. But when two elements are kept separate before us, it is very difficult,—in fact almost impossible for beings who have to think lineally or progressively,—not to put one of them before the other. There is almost always a sort of preponderance of one over the other, which decides which of them shall take the precedence. In the extreme case, to be presently discussed, in which no difference whatever can be detected between them in this respect, we should not naturally, —at any rate, not with perfect propriety,—adopt the predicative form at all. But if we do adopt it we should find one of the two elements would have to be forced into the first place, though it would now be optional with us to put whichever we pleased into that position.

Must then the subject of the proposition, occupying as it

does the prominent place in the mind, be necessarily the notion which contains the majority of the attributes, or the most important ones: be, if one may so say, the heavier notion of the two? There is a natural propriety in such an order of precedence, and where there is a substance in question we shall generally find that this is put into the first place as a matter of right. But there is certainly no invariable rule even here. For instance, when it is a matter of identification of an object by some observed characteristic, the object may very properly be put into the place of the predicate. Thus, in 'That white spot over there is a snow mountain', the subject here seems to owe its dignity of nature and position to its certain presence to the senses at the time. The great majority of the attributes here, and indeed the particular substance underlying them all, are remote and inferential, so that the single element which is unquestionably present takes precedence of them. In such cases the predicate may even be an individual with all its innumerable qualities, and yet be, so to say, strung on to a single sensible impression:—thus, 'That blue patch is Snowdon'. I do not think that in such a case as this last we could fairly be considered to be merely predicating a *name*, that is, the proper name 'Snowdon', of that blue sensible impression. This might be so, if we were now for the first time imposing a name upon what we see; but when, as here, the name is fully recognized as denoting a certain well-known object, all that we are doing is to identify the visual presentation entertained at the moment with the material object denoted by the name; or rather to refer the former to the latter. It is the predicate here which forms the great bulk of the synthesis involved in the proposition.

Must then all sentences have a subject and a predicate? In other words, neglecting merely grammatical considerations, which turn mainly upon the order in which the words are placed in the sentence, must there always be two elements present of which one possesses the kind of preeminence just indicated? Certainly not; several kinds of proposition may be suggested in reference to which the logician could detect nothing of that distinction which is the foundation of our recognition of subject and predicate.

For example, there is the case of pure synonymes, when we have two names for precisely the same object:—Plovers are Lap-

wings: Clematis Vitalba is Traveller's Joy. Here it does not appear that there is really any process of mental analysis and synthesis. Whether we penetrate below the mere name or not:—whether, that is, we think only of the names, and recognize that they are synonymes for an object with which possibly we have no further acquaintance, or whether we picture to ourselves the individual and recognize that both names are equally applicable to it;—in either case there seems no such distinction between the two terms that the logician ought to claim one rather than the other as a subject or a predicate. Of course we can put whichever of the two we please in front of the sentence, and thence make it, in the grammarian's sense, the subject, but this does not alter their essential equality. What the statement really means is that a certain object has two different names belonging to it. So regarded, we at once see the materials for a true subject and predicate before us. The bird or plant in question is the true subject, and the fact of having those two names is predicated of it, just as we might predicate any other habit or characteristic. But this is, of course, to change entirely the form of the sentence; so long as it was kept in its original form we could hardly say that any such distinction as that between subject and predicate was applicable to it.

With these synonymes must be ranked statements of equality, and indeed most estimates of ratio or comparison. If I say, 'This tree is larger than that', it seems very doubtful whether the mental process is best described as one in which we select either of these notions or objects and modify it by the other. We *may* do this if we please; that is, we may think of one of the trees, and amongst other attributes assign to it that of being larger than the other. But if we enquire what is the actual attitude of the mind towards the objects it is surely that of just placing them side by side and comparing them, and there seems not the slightest reason for saying '*A* is larger than *B*', instead of '*B* is smaller than *A*'.

The sum of the matter seems to me to be briefly this. Every proposition involves a synthesis of two or more elements. In the great majority of cases one of these elements will be larger or in some other way more important than the other, and this will naturally be put first, and, so to say, held up before

the mind during the time in which the other is added on to it. This forms what we may call the foundation of the relation of subject and predicate, so far as the facts are concerned. When we look at the mental side, we see that we cannot well help putting one object before the other, and uttering the words successively, and this seems to give the ground of the grammarian's view of the matter. The natural result follows, namely, that this form of speech comes to be habitually used in cases in which the physical foundation for it is no longer appropriate,—as when we say, Lapwings are Plovers, thus predicating one synonyme of another. There can be no doubt that it is always open to us to adopt this form, and for purposes of mere Logic often convenient to do so. Hence the practice of the formal logicians to insist upon every proposition being couched in the form *X* is *Y*, which so often seems to do violence to common conventions, and even to common sense.

There is another way of regarding the matter before us, which, being an unusual way may serve the better to direct attention to some of the points involved. A proposition may be regarded as the answer to a question actual or conceivable. Given then the answer,—that is, any proposition having been assigned,—in what different ways might we suppose the question to have been put so that this shall be the answer to it? Assume, for instance, that anyone on coming suddenly into a room hears the bare statement, 'Balfour is a statesman': upon what possible enquiries might this statement have followed? Three cases seem to be possible.

For one thing, the subject of the sentence, as it thus stands, may have been present to the mind of the enquirer, whilst the predicate was what he sought. That is, he may have been thinking of Balfour, and have been in want of some designation, in other words of some predicate to attach to him; and accordingly have asked, What is Balfour? Of course the missing predicate must have been confined within certain narrow but recognized limits, such as those of a state or parliamentary kind, but within these limits we may be supposed to be in ignorance. What was known and present to us was a subject; and what we wanted was some predicate for it.

Or again; a subject may have been sought for some known predicate, by the enquirer having asked, Who is a statesman?

Attention is directed to this case, as it seems to throw some light upon the real characteristics of the distinction between the two elements. The predicate here is clearly first in the mind in the order of time; in fact there it stands, waiting, possibly in vain, for its desired subject. But as soon as the mind has got possession of both the elements, we see that the person is the more important, and therefore the determining one, whilst the comparatively less substantial qualification or character is the one which is employed to supplement it, and therefore takes the secondary place.

A third possibility, of course, is that both terms were already present to the mind, the only doubt being whether they should be joined together; that is, the supposed question might have been, Is Balfour a statesman? Any proposition may of course be apprehended doubtfully; and the ordinary grammatical form for expressing such doubt, coupled with a desire to have it dispelled, is that which we call a question.

IV. *Affirmation and Negation.* For logical purposes, the distinction between affirmation and negation ought not to offer any real difficulty. In fact the perplexities which unfortunately have sometimes crept into the subject have mainly arisen from an almost wanton love of subtlety or paradox on the part of logicians.

The simple and obvious distinction which forms the ground of the separation of propositions into affirmative and negative, in itself needs no explanation, at least when we are dealing with the class of propositions which are most suitable for the predicative form. The predicate element which is, so to say, held out before us for the moment, for comparison with the subject, must either be found in it or not; it must either be a constituent member of the group or not. If the proposition be what is called an analytical or verbal one the alternative before us is, either that we are making an affirmation, and have extracted the predicate from a group of which we already know it to be a member; or we are making a negation and have looked in vain for some element which we knew not to be there. If the proposition be a synthetical or real one, the alternative is that in affirmation we are going to add on some predicate to the group, whilst in denial we equally hold it out before us and say we are not going to put it there.

The only point here which seems to deserve any notice is that one particular difficulty which we saw might attend affirmative predication is absent from the negative form. We saw that affirmation consisted in separating one member from a group to which it belonged, and yet in regarding, or rather in naming, that group as if this member were still there. We found in fact that in certain cases where the predicated members constituted a large proportion of the total group, the predication was apt to assume an almost fictitious form, as for instance when we spoke of the weather by enumerating all the attributes which constituted it. From this anomaly negation is generally clearly free. I may picture as many qualities as I like which are *not* to be found in the subject group, and it is obvious that in so doing we are in no way tampering with the integrity of that group or reducing it to a fiction.

The way in which the ingenuity of logicians has been most successful in creating difficulties here is in trying to evade the fundamental distinction above indicated by employing one form for both affirmation and denial. This it is attempted to do by the use of negative predicates, retaining only one form of copula. Thus, instead of saying, The stone is not black, some would phrase it, The stone is not-black, thereby securing the semblance of an affirmation. We need hardly pause to point out that nothing is effected in this way except so far as the mere expression of our propositions is concerned. Recur for a moment to what was said when we were dealing with the distinction between positive and negative terms. We saw that 'not-black' denoted the whole class of things of which 'black' was denied, or could be denied; its connotation or characteristic distinction was the absence of black, and nothing else. To assert the absence of black, or to deny its presence, are merely varying verbal expressions of one and the same fact.

In itself, any such attempt to do away with the form of negation by transferring it from the copula to the predicate in the hope that it could better be stowed away out of sight in the latter than in the former, is an entire failure. But it may serve to direct us to two considerations of some importance.

(1) Fully admitting the merely formal character of the transformation in question, there is a certain development of Logic which, dealing exclusively with the form, finds it con-

venient to adopt this transformation. This is that development of Formal Logic which I have proposed to call Symbolic. The object of adopting this course, in this special treatment of the subject, is to secure simplification. Whereas common speech makes use (universally) of positive and negative copulas, and also makes use (not unfrequently) of positive and negative terms as well, the Symbolic scheme finds it economical to employ one form only of copula,—at least so far as universal propositions are concerned,—namely, the negative form, marking the distinctions of affirmation and negation in the predicate. To carry this out it adopts a thorough-going *class* interpretation of every proposition, declaring that such and such a class is empty, that is, non-existent. Thus when it wants to assert that, All cruciferæ are edible, it frames the statement in the form, There are no cruciferæ non-edible; that is, it declares this compartment to be unoccupied; this potential class to be non-existent. If it wanted to state that, No orchids have opposite leaves, it frames the statement thus, There are no orchids opposite-leaved.

The transformation thus made use of is adopted entirely for simplification, and for the symbolic power which comes of simplification. No one who uses it should ever suppose that he is abrogating the fundamental distinction between affirmation and denial of any specified fact, however optional he may have rendered it which form shall be preferred for the purposes in hand.

(2) The other point to be noticed is that though the distinction between affirmation and negation is always peremptory and unambiguous, yet it may often be difficult in any given case to say which of two propositions best deserves to be called the affirmative. By this it is meant that given a proposition which we have decided to consider as being intrinsically affirmative, it is easy enough to state the corresponding negative, and conversely; but the decision, as to any single proposition, whether it is most reasonably to be described as positive or negative, may often be a difficult decision. The antithesis, in fact, often comes to be a relative rather than an absolute one. If we revert to what may be called the typical primitive case of predication, viz. that of a substance with simple attributes, here indeed there is seldom much difficulty. As between the statements that the picture is square, and that it is not square, no verbal arrange-

ment would disguise the fact that the former is the affirmative. But as between the statements that a certain mountain summit is accessible and that it is not accessible, the case does not seem so plain. When we picture the facts implied in the statements we can do so by imagining a climbing party reaching the top after many efforts, in the one case, and turning back or falling down in the other. Is the latter intrinsically more of a negation than the former?

The fact that it should be so difficult to give an offhand answer in some of these cases reminds us how widely the conception of predication must be extended. We may begin, as was pointed out at the commencement of this chapter, by taking substances and their sensible qualities, but we find that the same formal framework will serve to indicate syntheses of an exceedingly complex character. Now so long as we deal with such elements as colour or weight, the *non*-possession of these attributes offers a very wide and heterogeneous field as compared with their possession. The difficulty of saying which is, and which is not, in itself a negative quality is increased by the fact that the one is by comparison definitely limited whilst the other opens out an indefinitely wide scope. The distinction was clearly marked by the old logicians when they described terms which merely marked the non-possession of some quality as 'infinite'. They were right enough as regards the bulk of such simple attributes as mostly present themselves in common life, but we should greatly underrate the range over which the act of predication may extend if we were to suppose that this comparative indefiniteness must always be the result. The non-possession of a quality may really, when we look at the facts denoted by the terms, in certain cases be the simpler conception of the two.

CHAPTER IX.

THE SCHEDULE OF PROPOSITIONS.

HAVING so far discussed the general nature and functions of Propositions we must next enquire into their subdivisions; that is, we must ask how many kinds of propositions there are, and how these stand related to each other. We take it for granted that the reader knows something of the familiar logical doctrines on this subject; and therefore instead of spending the time in an exposition of the common view we will rather work round about the subject, raising such questions as these, How and why does it come to pass that there should be just four forms of proposition generally accepted? Are there any other systems of logical science which would naturally lead to a different scheme of propositions? In particular, is it necessary for inductive purposes to enlarge the accepted scheme? and so forth.

I. First then, as regards the origin and justification of the familiar traditional scheme. This scheme seems unquestionably to have been suggested by, and indeed selected from, the propositions of common life; though, as we shall presently see, it comprises but a small proportion of those actually in every-day use.

In order to settle this point a previous question must be raised, which is best phrased as follows. In what way,—that is, in what kind of form, whether of substantive or adjective or otherwise,—do the subject and predicate present themselves to the mind when we make assertions? This question, like many others raised in Logic, subdivides into several branches, according as we enquire (1) how the matter is decided spontaneously, that is, how the logical habits implicit in human thought have decided it already for us: (2) what is the most convenient arrangement to adopt for ordinary logical purposes: and (3) what

other possible arrangements there may be available for this or that special logical procedure or system.

(1) What then is the way in which the subject and predicate present themselves to the mind when we have no theories consciously before us, and are unbiassed by any wish either to conform to logical usage or to invent new schemes? For my own part I have little doubt that, speaking generally, the subject of the proposition is most usually contemplated as a class, i.e. in its extension, and the predicate as an attribute, i.e. in its intension. This is one of those points which each must be left to decide for himself, as we should not too hastily conclude that every mind works on exactly the same lines. Take an example or two. If I say that 'all diamonds are combustible', I am joining together two connotative terms, each of which therefore implies an attribute and denotes a class; but is there not a broad distinction in respect of the prominence with which the notion of a class is presented to the mind in the two cases? As regards the diamond, we think at once, or think very readily, of a class of things, the distinctive attributes of the subject being mainly used to carry the mind on to the comtemplation of the objects referred to by them. But as regards the combustibility, the attribute itself is the prominent thing: it is, so to say, not merely caught up at once but retained as an attribute in order to be added on to the objects included by the subject term. Combustible *things*, other than the diamond itself, come scarcely, if at all, under contemplation. The assertion in itself does not cause us to raise a thought whether there be other combustible things than these in existence.

It is no doubt extremely difficult to say what takes place in the course of the lightning flash with which proposition after proposition flies through the mind. What we must be supposed therefore to refer to is not the semi-conscious or symbolic usage of our terms, but their first distinct appreciation. That is, we must conceive the process of assertion to be sufficiently deliberate, or the attention to be sufficiently concentrated on the process, for us to be able to state distinctly what we mean, without these changes having been carried sufficiently far for *inference* to have had time to come into play. If we once begin thus to supplement our first thoughts by introducing

inferences, or by correcting what we did actually think by what we consider that we ought to have thought, we shall soon disturb the proposed conditions.

As already remarked, this seems a case in which one person is hardly entitled to dogmatize for others, but each must be left to state his own experience. So far as I can judge for myself it would seem that the first glimpse of any conscious significance in the elements of the assertion consists in the presentation of a sort of representative diamond being burnt; but more than this is clearly necessary in order to appreciate the universal proposition. The moment we pause sufficiently to distinguish and accept the proposition as a universal one, we seem to outline, as it were, the class of diamonds; to perceive that there are others like the representative one; and to recognize that we are to take every one of these into account. To do this is to interpret the subject in its extension. On the other hand it does not appear that any such reasonable pause leads one to treat the predicate in the same way. We do not feel any necessity, not any wish indeed, to outline the corresponding class of combustibles. If any one definitely asks us whether there *are* other combustibles, we should of course say that we know there are; but this is a digression and no part of the statement. In other cases we might say that we do not know, and for the purpose in hand do not care, for that we are merely thinking of the quality itself and not of the area over which it extends.

Of course there are plenty of exceptions to the above statement; cases, that is, in which the class-reference of the predicate will be no less definite than that of the subject. For instance in the classificatory sciences there is often little or nothing to choose between the two elements of the proposition in this respect. Thus, on hearing the statement that 'all tigers are felidæ'; if the latter term conveys any clear meaning to the mind at all it will probably stand upon much the same footing as the subject. Each of these classes is doubtless determined by its appropriate attributes, by which it becomes capable of definition; but each is presumably so constantly referred to, by those concerned with it, in the shape of a definite class of objects, that it will very likely present itself in this shape rather than in that of an attribute or group of attributes.

Corresponding to this relation between the subject and the predicate in a proposition there seems to be a very similar relation between two parts of speech, viz. the substantive and the adjective. Without attempting any critical or philological analysis of their origin and significance, the following hint may be suggested. Recalling what was said, in the last chapter, about connotative or class names, we saw that every one of these names corresponded to a class actual or potential, but that there was a broad difference in one respect as regards the familiarity of these classes. Sometimes the objects composing the class have many attributes in common, and therefore have to be viewed together under many different aspects; sometimes, on the other hand, they have but one, or very few, attributes in common, and therefore present themselves under but one or few aspects. Groups of the former kind have naturally much more mutual cohesion, and might often hold well together without the internal bond of any common name, whilst those of the latter kind seem to stand in need of some such help if the mind is to retain them as a unity. The former are as a rule indicated by the names which we call substantives, and the latter by adjectives.

To the grammarian the main distinction between substantive and adjective is found in the fact that the former can, and the latter cannot conveniently stand as subjects of propositions. This seems only another way of stating the fact just above mentioned, namely that the subject and predicate are very differently appreciated or interpreted in the mind. If it were at all an adequate account of the matter to say, with Hamilton and others, that in a judgment or proposition two notions are compared together and seen to be congruent or otherwise, it is hard to understand why these notions should not simply be reversible, still using the same terms to express them. But this is often what we cannot do, and the fact that we cannot do it is the cause of a certain friction between the logician and the grammarian. For instance, in the process of conversion, it is just as easy to say in symbols 'some X is Y' as to say 'some Y is X'. But when we put this into words we very often find that it will not do without some change, because the X and the Y had been respectively expressed by the substantive and the adjective appropriate to their initial relation

as subject and predicate. Thus 'some men are ingenious' goes very awkwardly when converted, unless we frame it in such a way as 'some ingenious beings are men', or use an equivalent rendering. And so in most other cases, the adjectives which can play the part of a subject effectively being decidedly exceptional.

The reader will understand that these remarks about substantives and adjectives are only offered here as confirmatory of our present position, that the subject and the predicate of the propositions of common life do really present themselves in a somewhat different guise; the former as a general rule inclining towards the extensive or class interpretation, and the latter with equal generality inclining towards the intensive or attributive interpretation.

(2) This being so, what sort of division or schedule of propositions would seem to be most natural? We might raise this question in respect of either subject, predicate, or copula. As to the subject, remembering its primary class-reference, we should certainly look for a two-fold arrangement according as we took the whole or a part of the class, and should also proceed to ask what significance was yielded by resolving to take *none* of it. As to the predicate, with its primary attributive reference, we should recognize only two alternatives, namely those of acceptance and rejection. And as to the copula, since its only business, under present limitations, is to assert or deny, its significance is already given in the two alternatives open to the predicate.

When the subdivision is thus carried out, the final result seems to be in accord with the four-fold scheme so long familiar to logicians. Inasmuch however as the way in which this accordance is secured does not seem quite obvious, and the intricacies which lurk at many points of the path do not always receive the notice they deserve, we will discuss the question rather more minutely than would otherwise be desirable. We will begin with the so-called *quantity*, that is, with the modifications of the subject.

(i) *To assert of all*, is intelligible enough: at least it does not seem to involve any difficulties beyond those inherent in every account of cognition. The natural and simple expression for it would take the form 'every X is Y', or 'all X is Y'; that

is, the familiar universal affirmative. So far logic and common usage are in perfect harmony.

(ii) *To assert of some*, begins to open up difficulties, for here we find the paths of logic and of common usage commencing to diverge. What the plain man invariably means by 'some' is 'some only', but certainly more than one. And in both points he would be confirmed in his interpretation by the usage prevalent in a law court, where it is essential to keep close to popular renderings of popular terms. At least a witness who should swear that he had seen 'some' of the prisoners at the bar near the scene of a crime, when he had really seen them all there, would be pretty certainly reprimanded, and asked why he did not say he had seen them all. And if he swore the same when he had really seen only one of them it would fare still worse with him. In both these respects logicians have almost invariably agreed upon a special interpretation. They have decided that 'some' shall have a very wide range, extending downwards so as to embrace 'one' only, and upwards so as to embrace 'all'. In a word they interpret it to mean *not none.*

The full reasons for this interpretation will only gradually appear. But it may be remarked that we thus greatly simplify matters, for we are enabled to reduce the six resultant forms suggested on the last page to four. But the reader must carefully notice that on this rendering the particular proposition *includes* the general one. This seems at first sight paradoxical, for it would generally be said, on the contrary, that 'all X is Y' includes 'some X is Y' under it. Regarded as objective facts this is doubtless so, in the sense that every 'some' of the class is a part of 'all' of that class. But regarded as an *assertion*, it is the other way. 'Some X is Y', as commonly interpreted in Logic, is the general expression which may possibly include 'all X is Y' as a single case, viz. the limiting case, under it.

(iii) *To assert of none.* Language is so entirely an affair of convention that it would be idle to spend much time in enquiring what a phrase might have meant, when we very well know what it does mean. Moreover on this particular point it is difficult to say anything without getting on to ground which has long been suitably occupied by venerable jokes.

So it may just be remarked that since 'all X is Y', and 'some X is Y', mean respectively the attribution of the predicate to the whole and to a part of the subject class, 'no X is Y' would seem, if we thought only of verbal consistency, to attribute it to none of the class, that is, simply not to attribute it at all so far as this class is concerned. Now this would be a different thing from denying it of the whole class, for the mere negation of an assertion is a different thing from setting up a counter assertion. This we admitted in the case of 'some X is Y' which we expressly confined to the asserted 'some', by saying that it involved no negation of the remainder after that 'some' was accounted for.

In other words, if in 'All X is Y' we start with 'all X' and predicate Y of each member; and in 'Some X is Y' we start with 'some X' and do the same; ought we not in 'No X is Y' to start with 'no X', and predicate Y of this: in other words not commit ourselves to any assertion whatever?

Convention, however, in the exercise of its supreme right, has decided otherwise, and has laid it down that the phrase intrinsically appropriate to assert of none shall be taken as exactly equivalent to denying of all. That is, 'No X is Y' is understood to deny Y of any and every X.

(iv) *To deny of all.* Here again if we were reconstructing language in accordance with what seems suitable we might suggest as an appropriate expression, 'All X is not Y'; on the ground that we first think of 'all X' and then deny Y of it. And several logicians have actually employed this expression for this very purpose. But usage has been too definite and stubborn to be successfully set aside, and accordingly the phrase last considered ('No X is Y') has been generally adopted for denying of all, whilst this form ('All X is not Y') has been sedulously avoided by most logicians. Their reason is that popular usage has made it exactly equivalent to another expression which they prefer, viz. 'Some X is not Y', which comes to be noticed next; and it is of course confusing to recognize two distinct but equivalent phrases.

(v) *To deny of some.* Here it seems to me that the paths of common and of logical usage again run into one track. 'Some X is not Y' is universally, and perfectly appropriately used by us all, whether logicians or not, in order to deny an

attribute of 'some' members of a given class, the only divergence consisting in the limits of the 'some'. As just above remarked popular usage also employs, as an equivalent rendering, the form 'All X is not Y', as for instance in 'all indulgence is not allowable', which simply means that some is not.

(vi) *To deny of none.* For this we should presumably, if asked to suggest a phrase offhand, offer the following,—'No X is not Y'. This however has found very little acceptance either in popular usage or in logical manuals. So far as it has effected a footing, however, it seems to be accepted in a sense analogous to that which we found was imposed upon the 'Affirmation of none'. That is, 'No X is not Y' is understood in a contrary, viz. in an affirmative sense. It does not, by denying of 'nothing', tell us nothing. Just as to 'affirm of none' was interpreted as equivalent to deny of all, so to 'deny of none' is interpreted as equivalent to affirm of all. Hence this expression also becomes superfluous like the fourth.

The outcome of all this may be briefly stated thus. A class interpreted in extension, with a three-fold division,—i.e. a class as to which we can assert or deny of all, some, or none,—yields six possible forms. On examination, however, it is soon seen that there are only four really distinct meanings commonly held, two of the forms being only retained as equivalent or alternative forms to two of the others. Hence the resultant four well-known forms of proposition, viz. All X is Y, Some X is Y, No X is Y, Some X is not Y.

These forms are most instructively arranged as follows:

All X is Y contradicted by Some X is not Y.
No X is Y .... Some X is Y.

A different, and rather elaborate, technical arrangement and nomenclature for the mutual relations of these propositions is given in the handbooks, under the heads of Opposition and Conversion. For our present purpose however it is better to arrange them as above. We are thus reminded that the two forms in the first column are the primary, in the sense of the most important, forms. They are the only forms with which science can consent to be permanently satisfied, the others existing as temporary expedients. This they may do in two ways, either as corrections of the corresponding universal on the same line with them, which therefore (if established) they

simply controvert and remove; or as commencements towards the other universal.

Suppose for instance that we are dealing with the assertion that 'some climbing plants revolve from right to left'. For scientific purposes we can never contentedly admit such a statement into a place alongside of universals, as the formal logician does. It is to us merely a temporary step to something further. Thus it may possibly come into direct conflict with a previously accepted universal that no plants so revolve; which, therefore, on due proof it upsets. But can it be regarded as a stepping stone to the corresponding universal, viz. that *all* such plants revolve from right to left? Certainly we might hope eventually so to regard it, unless we were faced, as we should be in matter of fact, by the corresponding particular negative, that 'some plants do not revolve from right to left'. If this were the case, both these particular propositions would have temporarily to be set aside or reserved for examination until they could be reduced to universals on a smaller scale. We feel quite sure that there must be some characteristic, though we have not yet detected it, which would enable us eventually to assert that 'all climbing plants of such and such a description revolve from right to left', and that all of such another description revolve in the opposite direction.

Suppose, again, that we meet the assertion, or acquire the information, that 'some plants are built up of cells'. Here again we cannot rest. We have no counter-universal in this case to rebut; accordingly we start with a sort of slight presumption that such a particular may go on to become a universal. What we aim to do is to generalize it into '*all* plants are built up of cells', or, if not so much as this, at least into the more specific assertion that all plants of such and such a description are so built up.

We call attention to this, as to other points of divergence between the common and the inductive treatment of Logic, because they are liable to be overlooked. In the former,—beyond the slight slur conveyed by the words "pars deterior" in the syllogistic rules,—particulars and universals seem to stand on a footing of equality. But to us it is quite otherwise. Truly particular propositions,—such, that is, as fulfil the ordinary logical conditions,—are essentially unscientific. They

can at best be regarded as temporary resting places in our attempt to generalize and obtain universal propositions.

In thus speaking of particulars as being unscientific it must not for a moment be supposed that we deny their existence as temporary and justifiable halting places. Jevons has gone too far in this direction. Feeling how troublesome their management was in the usual symbolic treatment of Logic, he endeavoured to rid the science of them. His position, so far as I understand it, is this. He maintains that there is always at bottom some qualifying characteristic in a particular proposition which would enable us, if we knew all the facts, to express it as a universal: that, in fact, 'some X is Y' is always in reality 'all X that is Z is Y[1]'. This appears to me to involve some confusion of ideas,—one of the many such which arise from not distinguishing between the true subject-matter of Logic, viz. our assertions about facts, and those facts themselves. It is quite true that 'some X is Y' may eventually be thus expressible as a limited universal, just as, for that matter, everything now unknown may eventually be known and expressible by us. But what propositions are bound to assert is what we know and mean at the time, and no more than this.

Now there are plenty of ways in which the truly indefinite particular may be acquired, and may demand expression as such. We may, for instance, have acquired it from authority. Some one may have told us that it is not true that 'No X is Y', and then our position is exactly hit off by the affirmative particular. Or we may have got at the fact deductively. If I know that more candidates entered than succeeded in passing, I know that 'some were plucked', but it does not follow that I have the slightest ground for knowing what was the characteristic limiting mark required in order to convert this into a narrower universal. Again I may have observed after a frost that certain known plants in my garden were killed, but it does not follow because I say 'some plants are killed by frost', that therefore I must have only these in view. I may distinctly intend to generalize somewhat, and to convey the belief that a greater number than those I actually saw, were so killed. I may

[1] So that, symbolically, in his notation, it stands in the form $ZX = ZY$ where Z is not truly indeterminate, as Boole maintained in his corresponding form, but a class term subject to the same laws as X and Y.

realize the proposition, and intend to communicate it, in a truly indefinite form. And so in many other cases. For these reasons therefore the particular proposition cannot reasonably be rejected from the province of Logic.

II. So much for the traditional scheme of elementary propositions. It consists really of a selection, with but slight modifications, from amongst the popular forms of speech; these last taking their shape and arrangement, as we have seen, from the prevalent habit of translating the subject in respect of extension and the predicate in respect of intension. We must now enquire what sort of schedule would be yielded if we insist upon interpreting both subject and predicate in respect of their extension.

To begin with; both subject and predicate will now have to be divided, as the subject alone was divided before, in a threefold way, according as we take all of it, or a part, or none of it. So much is clear. But when we come to ask what has to be substituted for the affirmation and negation adopted in the previous scheme, we find that the change is important. We cannot strictly predicate one class of another. In respect of their extension one term can only be included in, or excluded from another. Accordingly the question must be modified, and stated in the following form,—In how many distinct ways can two classes, denoted respectively by our subject and predicate, stand to one another in respect of total and partial inclusion and exclusion?

I have discussed this point elsewhere (*Symbolic Logic*) with some minuteness, and will therefore only give the results here in a very summary way. It appears, then, that there are five such relations, viz. the following,—

The classes X and Y coextensive,
The whole class X coextensive with part of Y,
The class X including the whole of Y and more,
The two classes partly coincident and partly distinct,
The two classes wholly distinct.

As regards the verbal rendering of these relations there is a simple and complete mode of effecting this, demanding however that the word 'some' shall be interpreted in the distinctive sense of 'some, not all'. We can accurately express them by the five propositions,—

All X is all Y,
All X is some Y,
Some X is all Y,
Some X is some Y,
No X is any Y.

One or two points may be noticed in connection with this scheme. In the first place this five-fold arrangement is precisely equivalent to that which is so familiar to us in the well-known Eulerian diagrams, viz. the circles so often employed to illustrate propositions and syllogisms. And the fact that they do so accurately fit in with these diagrams is to my thinking a proof that the diagrams are not very suitable to illustrate the common scheme of propositions. A four-fold scheme of propositions will not very conveniently fit in with a five-fold scheme of diagrams, as the reader will soon find if he tries adequately to represent, say, the particular negative. Again, it must be noticed that what the five propositions (or their corresponding diagrams) are competent to illustrate is the actual relation of the classes, not our possibly imperfect knowledge of that relation. By this is meant that when we say 'All X is Y' we may not know, and certainly do not announce, whether or not the X covers the whole of Y. But when we turn from this common proposition to the actual facts underlying it, we see that X must (under the conditions so stated) cover all or a part only of Y. And the five-fold scheme forces us to choose one or other of these two, since it offers us no single form which expresses the alternative between the two statements, 'All X is some Y', 'All X is all Y'.

III. There is a third account of the arrangement and division of propositions which may be offered, which, like the last, must be very briefly noticed here, as its application leads us into another direction than that most appropriate to Inductive Logic. This is the account in accordance with which propositions are regarded as *existential*, that is, as asserting or denying the existence of things corresponding to a certain term or combination of terms. Every proposition may be so regarded; though the alteration in their rendering, and the consequent disturbance of customary schemes of arrangement, will seem rather startling to those who examine them for the first time in this light. Thus 'No X is Y' is interpreted as denying the existence of any such

class as XY; 'All X is Y' as denying 'X that is not Y'; and so forth. Among the many variations entailed by the acceptance of such a scheme one of the most remarkable is that the really fundamental distinction between propositions becomes that between the universal and the particular. All universal propositions, whether affirmative or negative in their customary form, are here interpreted as negative. That is they deny the existence of a certain combination. On the other hand all particular propositions are interpreted as affirmative; that is, they declare that a certain combination does exist.

For a full discussion of this scheme of propositions, and a criticism of its strong and weak points, the reader must be referred to the proper quarter[1]. Such a scheme is the basis of most of the so-called 'mathematical' treatment of Logic, and adds enormously to our power of grappling successfully with complicated propositions. In fact, groups of really complicated propositions cannot easily be combined, and their net result completely determined, on any other scheme yet worked out. But in spite of this, or rather as a consequence of this, such an existential rendering of the proposition does not seem to me a very suitable one for Inductive purposes; and I shall therefore adhere to the traditional form of proposition, so far at least as any technical form requires to be employed.

In speaking, as we have been doing, of three distinct renderings of the import of a proposition, and the consequent distinct schedules of propositional forms which have to be drawn up, the reader must be on his guard against a possible misunderstanding. There is no question here of right or wrong; we are not now deciding between the claims of hostile theories. Nothing more serious is at stake than a question of convenience and of efficiency of method. There has been far too much of a disposition on the part of logicians to consider that there must necessarily be some one correct view as to the import of propositions, and that therefore in deciding

[1] The question is discussed at length in my *Symbolic Logic*. It ought to be pointed out that this particular employment of the existential form is characteristic of the Boolian system, and of the methods of those writers (for instance Jevons and myself) who adhere more or less to his general conceptions. Dr Keynes has adopted a somewhat different method; and others, like Mr Maccoll, start from a very different point of view.

for one they must reject others. They have always retained something of the theologian's spirit.

It must be strenuously insisted upon therefore that any propositional rendering and scheme which we may adopt is a somewhat technical and artificial selection of our own. It is not that we invent a new form, or decide that one only out of many is right; it is rather that out of the many and various, but more or less equivalent renderings adopted by common speech, we select one as most efficient for our purpose in hand. Having done this it becomes more consistent and scientific to adhere to it throughout; that is, to substitute, in every case, our equivalent proposition in place of any of the other forms which may happen to present themselves.

For instance, as regards the popular recognition of all these, and probably many other forms of proposition, abundance of illustrations may be offered. For the traditional four forms, indeed, no examples need be offered, for they are far too familiar to need this. But as regards the two other views noticed above a few instances may be conveniently offered. Thus, as to the class-inclusion-and-exclusion view, we should just as naturally say 'lawyers include solicitors',—(technically, 'some lawyers are all solicitors',)—as use the two ordinary propositions which make up the same statement, viz. 'all solicitors are lawyers' and 'some lawyers are not solicitors'. Again it would be as natural to say, 'Gravitation and inertia are co-extensive'—(technically, 'all gravitating bodies are all inertia-possessing bodies')—as to employ the two universal affirmatives demanded to convey the same information. Plenty of other examples might be offered, but these are sufficient to remind us of the thorough practical recognition of the class explanation in every case where it will convey our full meaning most readily.

Then, as regards the existential interpretation, a similar abundance of examples readily offers itself. It is just as natural to say 'There are no non-conducting metals' as to say 'All metals are conductors'. It is more natural to say 'There is nothing which is at once cheap, good, and beautiful' than to adopt one of the subject-and-predicate equivalents, such as, 'No good and beautiful things are cheap', and so on.

The reader must not be induced to suppose, by the use of

this word 'existential', that we are going to snare him into any sort of discussion upon the nature of existence in an ontological sense. The sense in which the existence is to be understood, for logical purposes, must be supplied by himself, or be gathered from the intention of the speaker, or from the context in which the statement occurs. This point has been urged before. It has been already insisted upon that in every proposition without exception, if it be intelligently accepted or rejected, the distinction between reality and unreality, between existence and non-existence, is in some signification or other taken for granted. We cannot assert or deny (as has been abundantly shown already) without presupposing that this distinction has been already admitted and appreciated, though of course the interpretation adopted in various cases may be very different. One person may supply the common physical or experiential test, another some kind of conventional test, whilst a third may take that of mere conceivability; but that they come provided with some such test or standard must in every case be taken for granted.

In discussing these existential propositions we shall find it convenient to treat separately two somewhat distinct types of them, viz. those in which we start with a notion or term and claim existence (in the appropriate sense) for this, and those in which we might be said, without any violent stretch of language, to start with an assertion of existence and proceed to add on the notion or term required to complete the meaning.

(1) Propositions of the former type are not very frequent, and where they do occur they are mostly devoted to the emphatic declaration of actual objective existence as possessed by the subject of the statement. In fact this is about as emphatic a way of making such an assertion as our language affords, as for instance the proposition 'God is'.

In such cases as these, then, we may fairly say with Mill that a certain ambiguity is displayed in the use of the logical copula 'is'. In its normal application it covers the whole extent of mere logical predication, whereas here its significance is so contracted in range that it is used to imply actual objective existence. The proposition in question is exactly equivalent to 'God exists', and this is merely a logical abbreviation for 'God is existent',

i.e. we are here making a distinct predication about the Deity, that He is not merely a conceivable object of thought but one which exists outside our imagination and can have His existence verified in some way or other. In other words, though mere *logical* existence cannot be intelligibly predicated, inasmuch as it is presupposed necessarily by the use of the term, yet the special kind of existence which we call objective or experiential can be so predicated. It is not necessarily implied by the use of the term; it is not conveyed by the ordinary copula; and therefore it is a perfectly fit subject of logical predication. To say 'God is existent', if 'existence' here meant nothing more than logical conceivability or predicability, would be a mere pleonasm; but to make the same assertion in the narrower sense of existence, is to utter a perfectly consistent proposition, which contains subject predicate and copula, and in which the predicate and copula are not in any way synonymes. This may be made clearer by the following example of disjunctive reasoning:— "Whatever is, is either self-existent or created: the world is; therefore it is either self-existent or created." The accent necessarily laid here upon the first (and third) '*is*', indicates the special signification which it bears in such an application.

(2) The other class of existential propositions comprises those in which the notion or term occupies apparently the position of the ordinary predicate, whilst in the place of the subject stands the bare affirmation of existence of some kind, as in 'There is a devil'. With these we may class the ordinary impersonal propositions of the type 'it rains', when one of them is expressed—as it might be rather awkwardly expressed in English, or conveniently and accurately in some languages,—by the words 'there is rain'.

The general character of such propositions as these has been already discussed in the previous remarks about the nature and varieties of propositional statement;—or rather, for this is an important distinction, what we there discussed was the possibility of converting any predicative proposition of the common type into one beginning with this vague assertion of existence "there is", but concluding with a *complex predicate.* Thus we saw that 'there is no such thing as corrodible gold' was the existential equivalent for 'no gold is corrodible'; that, 'there is no gold which is not a good conductor' was the

equivalent for 'all gold is a good conductor', and that analogous propositions of an affirmative character were the equivalents for the two familiar types of particular propositions. We should seldom find any difficulty in throwing back an existential proposition of this complex kind into the customary predicative shape. There is, of course, an occasional ambiguity as to which of the two elements whose compatibility is asserted or denied is to be selected for subject and which for predicate, but this simply results in an alternative which we are at liberty to resolve at our choice. Thus 'there are no incurable lunatics' would naturally be thrown into the form 'all lunatics are curable', though if we prefer a negative rendering we may state it thus,—'no lunatics are incurable': besides these, the reader will easily perceive other equivalent renderings. So if the given proposition were 'there are deserving paupers' we should analyze this into subject and predicate in either of the forms 'some paupers are deserving', or 'some deserving persons are paupers'. All this is plain enough, and in the Symbolic Logic, where the existential form of proposition is adopted to the exclusion of all other forms, the consequences of carrying out this system to the furthest attainable development will be found discussed in all needful detail.

But this does not at first sight seem to throw any light on propositions which do not exhibit this complexity in the term which is to be resolved into both subject and predicate, such as the examples quoted above: 'there is a devil', 'it rains'. How are these to be treated? We must certainly somehow find the desired two elements, of subject and predicate, and I should seek them as follows. In the former of the two propositions we may regard the word '*is*' as open to analysis; that is, we may consider that it predicates existence of the objective kind, so that the proposition is transformed into 'a devil exists', or 'a devil is existent'. That is, such a proposition as this has no intelligible meaning or significance unless it predicates real or objective, as distinguished from notional or conventional existence.

In attempting to resolve such a proposition as 'it rains', with a view to making it yield us both a subject and a predicate, we shall find no difficulty if we bear in mind the essential complexity of all terms at bottom. It has been

already frequently insisted on that the fact of a single term being employed, instead of an aggregate of terms, does not raise the slightest presumption that the notion or mental synthesis is at bottom less complex. It is an accident of language, or of the more or less frequency or importance of the notion under given circumstances of time or place, whether one single term be available or whether we have to put together a verbal structure composed of many elements. Thus in this proposition, 'it rains'; no one can maintain that the fact asserted is any less complex that that which we naturally throw into a fuller form when we say 'the sky is overcast'. Indeed this latter assertion might itself be just as conveniently made in the impersonal form 'it is gloomy' or 'it is dark'. How arbitrary is the selection of language in this respect might be illustrated by innumerable examples. Thus both we and the Germans can say 'it rains' or 'it freezes'; we cannot, whilst they can, say 'it dews' or 'it sheet-lightens'; and we can neither of us say 'it auroras'. But who will assert that one of these phenomena is more complex, or has a better right than others to the full equipment of the predicative form of assertion?

If therefore any one asks us how the proposition 'it rains' is to be transferred from the impersonal or existential form to the predicative, we must reply that our language offers no very obvious or convenient substitute. But as the logician declines to be thwarted by such obstacles he need find no difficulty in splitting up the notion so as to make it yield him both subject and predicate. Thus he may prefix the word 'sky' or 'heaven', and predicate rain of this, which is just as good a meaning in itself as to attribute 'being overcast' to the sky, or 'lowering' to the heavens. Or, if he prefers to keep to the notion of rain, and to analyze this, he may remember that rain is falling water, and that therefore the idea is definitely expressed by 'rain is falling'. Anyhow he need never feel at a difficulty to render any proposition, however contracted be the form in which it is offered to us, into the full logical array afforded by the possession of subject, predicate, and copula.

IV. In discussing above the accepted forms of proposition we were purposely reducing them to a minimum, that is, enquiring what is the smallest schedule with which we can conveniently

work. Popular speech, however, contains a great variety of other forms, and it may therefore be fairly enquired what are the principles on which so many of these are excluded. Still more particularly must it be enquired whether the same exclusions are to be retained when we are preparing for the wants, not of the narrow syllogistic Logic but of a comprehensive Inductive system.

The main exclusion of the common system consists in the rejection of every other class comprehension than 'all', 'some' and 'none'. And even this restriction, as we have seen, admits of narrower application; for when we interpret 'some' as we always do interpret it, and take account of both affirmation and denial, we find that only four forms are actually wanted or employed, viz. the universal affirmative and negative, and the bare denials of these.

Popular speech, it need hardly be said, employs a multitude of other and intermediate forms:—'Many *X* are *Y*', 'nearly all', 'most', 'a few' &c. Sometimes it thus expresses them in an objective form, viz. as statements of the more or less frequency of events, or wideness of their prevalence. Sometimes it expresses them in the corresponding subjective form, as when we say of some particular event 'I am almost certain it will happen', 'I think it more likely than not', and so forth. (That these objective and subjective forms are almost exactly equivalent, I must content myself with asserting here; and must refer the reader to works on Probability to justify the assertion.) To all these intermediate forms of assertion the common Logic now rigidly closes the door. It used indeed in former days to admit them by a sort of side entrance, under the designation of 'modals', but finding them hopelessly intractable to such ordinary rules as those of Conversion, Syllogism, &c., it has now very prudently resolved to reject them altogether.

The only enquiry we have a right to make here is, whether any of these forms give ground for rational inference; that is, not necessarily for *certain* conclusions, but for conclusions sufficiently strong for practical guidance. This they unquestionably do; as follows:—

(1) For one thing, when propositions which assert or deny of 'few' or 'many' of a class are carefully examined, a uniformity will sometimes begin to present itself. It is the

peculiar uniformity mentioned in the fourth chapter, where it was described as comprising the basis of Probability. It yields propositions expressible in such a form as, 'on an average two out of every three X's are Y'.

To those who have once seized the true conception of a 'limit' in the mathematical and physical sense, there is really nothing more to be said about these propositions under the present heading. So far as their import is concerned they do not much differ from others, except as they show themselves peculiarly adaptable to the predicative as opposed to the class interpretation. They regard the subject in the light of a class and the predicate in that of an attribute; and they assert that in proportion as we go on taking more and more members of that class we more and more nearly find the assigned proportion of these members possessing the attribute in question. It follows from this that such propositions are of limited application, for they demand in full strictness that the numbers of members of the class should be infinite; and for practical purposes they demand that the members should be very numerous, as otherwise we should not allow free scope to the averaging agencies or influences.

There can clearly be no question of classifying such propositions, even in such a rude way as that of arranging them on the four-fold scheme of the common system. From the nature of the case propositions involving a numerical determination merge so insensibly into each other that it is obviously useless to think of grouping them. Indeed the distinction between affirmative and negative is almost evaded on such a view. Beginning with being nearly certain in favour of a proposition, when, say, we find that 99 out of 100 X's are Y, our confidence gradually declines as the proportion becomes smaller. When Y only holds good of half the X's, we are in absolute uncertainty about the truth of the proposition in any individual case. As the proportion declines still further our confidence diminishes continually; until when, say, only one out of the hundred X's is a Y, we are nearly certain that any given X is *not* a Y. That is, we have passed from the neighbourhood of positive affirmation to that of positive negation by a continuous process of diminishing the proportion of the subject class which possess the predicate attribute.

(2) Another class of propositions which we must admit is that of the ordinary arithmetical kind. The class here referred to is not that in which we determine the exact magnitude of some individual, or measurable quantity,—these will come under our notice in a future chapter, when we discuss the nature and use of *units*,—but that in which instead of having a proportion indefinitely approximated to we have an exact assignment of the proportion which has in a given instance been observed. For instance, instead of saying 'On an average two out of three men live to 50', we might say 'Out of 90 men born in such a town in the year 1800, 60 lived to the age of 50'.

Propositions of this type must of course be taken into account inasmuch as they enable us to draw conclusions. But so far as Logic, whether inductive or deductive, is concerned, they are apt to slip from our charge. If, on the one hand, we take them as they are,—that is, do not make them merely the basis of further inference,—they are treated by the ordinary rules of arithmetic. If I know that 60 men out of 90 lived to the age of 50, and that 20 out of the same number lived to 70, I know that 40 of them died between the ages of 50 and 70.

If, on the other hand, we regard these propositions as material for further inference they are apt to take their place amongst mere observations. By this is meant that propositions of the definitely numerical description, '17 out of 35 observed X's are Y', will for the most part be capable of becoming generalized into the statistical statement 'On an average 17 out of 35 X's are Y', before they come under serious treatment.

CHAPTER X.

THE HYPOTHETICAL AND DISJUNCTIVE JUDGMENTS.

FEW parts of Logic have given rise to so much controversy and diversity of opinion as that which deals with the nature and treatment of the Hypothetical judgment. This diversity displays itself at every point in the treatment of the subject. It shows itself in the usage of the technical terms employed, such as 'conditional' and 'hypothetical', which have been re-defined and interchanged by one writer after another:—in the account given of the nature of this class of judgments, and of their distinction from categoricals, including the question whether there be any such distinction at all:—and in the analysis of the so-called hypothetical reasoning, including the question whether there really be any such form of reasoning distinct from the categorical.

In such a state of things confusion can only be avoided by a sedulous adherence to method. Accordingly the alternative, which we have had forced upon us before, recurs again here. Which of the two plans are we to adopt: are we to start with the conventions of language and of thought as we find them embodied in common speech,—whether the speech of unsophisticated daily life, or the same as it has been selected and modified by common logic,—and do what we can to define these forms and extract from them the meaning they have carried down to us through ages of use? Or shall we reverse this order; that is, shall we penetrate below these forms, starting with those underlying facts of the world which thought and language alike have to deal with, and, analyzing these, endeavour to ascertain whether there is any class of phenomena sufficiently characteristic to give occasion to such a peculiar

form of speech as that which expresses itself in the various kinds of Hypothesis? As the reader will conclude, it falls in best with the general scope of this Treatise to adopt the latter course, which we now proceed to do.

Revert then to the logician's world, as we described and illustrated its salient features in the first chapter. As we there pictured such a world there would not seem to be any reasonable opening for more than one form of speech, which would naturally be the categorical. Many varieties of the categorical proposition would of course be needed. We should want the individual, particular, and universal, according as we were speaking of one, of few, or of all of a class. We should need past, present and future tenses; whatever view we might take as to the tense of the logical copula verb. These, and innumerable other modifications of the simple '*A* is *B*', would have to find a place. But where the entire range of events is,—under the explanations and reservations already laid down,—objectively certain, it is not easy to see how there should be a suitable opening for any of the many forms of speech whose typical commencement is 'If *A* is *B*'.

But it was fully explained that the world as thus contemplated belonged to a very remote stage. We are not there yet. The picture was an ideal which the logician had to postulate in order to make his theoretical position unassailable. The reminder is hardly needed that, however certain the world may be in itself, we do not yet know all about it; or, in stricter language, however invariable the uniformities may be, we have not at present secured hold of them all. In a word, we have to take account, in our scheme, of human ignorance and doubt.

It is this recognition of doubt, and the necessity of admitting it as a determining factor in casting the forms of human speech, which seem to me to afford a clue to the interpretation of hypotheticals. But as there are several different shapes in which this element of doubt may present itself, and consequently more than one linguistic form in which we have to take account of it, we must examine in turn the principal ways in which this element may be detected.

I. To begin with the simplest case possible. Any fact may be held doubtfully as well as certainly; or, in more

technical language, without altering in any way the matter or the form of the proposition '*X* is *Y*', we may either hesitate about accepting it or we may feel quite sure of its truth.

Here, then, seems an opening for a corresponding variation in our forms of speech, since the alternative involved is a very common and a very important one. The suggestion might, that is, present itself that whereas *certainty* about a fact, whether for or against, is adequately met by the categorical form, yet that the mere attitude of doubt about a fact, simply as such, calls for some appropriate linguistic form in order to distinguish it.

Now we do find some recognition of this distinction in popular language, for in one way or another we generally take care to make it plain what is our attitude in this respect towards the statements we utter. We have a whole catalogue of various qualifications: *X* may be *Y*; *X* is most likely *Y*; I do not know whether *X* is *Y* or not; and so on, in quite innumerable forms. And the older Logic, recognizing this, made a most painstaking but supremely wearisome and ineffectual attempt to incorporate the principle underlying these various forms into its schemes, in the doctrine of the so-called Modals.

Any arrangement of this sort, however, does not lead us quite in the direction we are seeking. What, on such a supposition as this, we are supposed to want is some peculiar linguistic or grammatical form set apart for the purpose in hand, analogous to, but distinct from, the categorical form. It is quite possible indeed that some day or other a form of such a kind may be required. When, if ever, we have advanced so far towards our ultimate speculative goal that the uncertain, instead of being the rule shall have become the exception, and when moreover we all find ourselves in tolerable agreement as to what is certain and what is doubtful, then perhaps we might find it convenient to adopt two quite distinct forms of expression indicative of two such distinct attitudes of mind. We are not there at present. What therefore the logician finds at hand is not a single peculiar grammatical form which he can contra-distinguish from the categorical, but an infinitely numerous array of qualifications of the categorical which take the shape mostly of mere additions to it, as when we say, 'I am not sure that *X* is *Y*'.

There is really nothing to be done with such a chaos as this. The attempts of the older logicians, above referred to, when they meddled with the notorious modals, is a warning not to try in this direction. The end they aimed at was right enough; it was their means that were insufficient. What we now do, on the other hand, is to make a distinction between those cases of doubt which admit of a numerical estimate, that is, which rest upon quantitative statistics; and those which have to be left vague and indefinite. The latter we let alone, leaving it to popular speech to exercise its resources upon their expression, and only hoping that we may some day be able, by enlarged experience, to bring them under quantitative measurement or estimate. As regards the former our attitude is very different. So far from thinking the old logicians wrong in attempting to reduce them to scientific rule, our only regret is that they wasted so much energy and ingenuity on hopeless methods. What we now do, it need hardly be said, is to relegate them to the science of Probabilities, the province of which may be briefly indicated, under its subjective aspect, by saying that it deals with inferences among propositions as to which our degree of doubt can be quantitatively estimated.

We must therefore refer the reader to some special treatise upon that subject. The topic is one closely connected at many points with Inductive Logic, of which it is indeed nothing more than a single department which has been highly developed. The principal reason why it is advisable to treat it apart is that its large and frequent appeal to mathematical principles and conceptions,—and indeed its large employment of detailed mathematical processes, if any accurate results are to be obtained,—demand a somewhat peculiar aptitude and training.

II. There is a second way in which we may entertain a measure of doubt in regard to the facts before us, and this under sufficiently frequent and definite conditions to offer a suitable opening for the use of a peculiar form of predication. We may, that is, know the *limits* of some intended assertion, but remain uncertain as to our position within those limits. This uncertainty may affect either the subject or the predicate. We begin with the latter, as the simpler case.

(1) We may know, then, that a thing possesses one or

other of two attributes, but have no means of deciding between the two. This leads to the commonest form of logical disjunction expressed in the form, X is Y or Z. Now there is no doubt that such a proposition as this seems very distinct from the categorical; but a little consideration will show that in many cases the difference is by no means serious. In the first place, remember that a proposition may be interpreted under its class aspect. When this is done 'X is Y' relegates the X somewhere or other within the range of Y, but does not indicate how much of the range is thus occupied. What more then is done by the disjunctive 'X is Y or Z', beyond referring X in a similarly vague way to the range of Y and Z combined? The indication of uncertainty is increased in the latter or disjunctive form, but it does not make its first appearance there. And even any such increase of uncertainty is due in part to the accidents of language. Suppose we have a single name for the class 'Y and Z', and substitute this, the expression may again become categorical in form without actual change of meaning. Thus, if I assert of a certain man that he is a lawyer, I refer him to a certain class of persons. But I do exactly the same, neither more nor less, if I say of him that he is either a barrister or a solicitor, provided it be known that these are subdivisions of the class lawyer. The latter statement, though sounding the more indefinite of the two, is not really so; all that it does is to emphasize a distinction which existed and might have been known before.

It must be admitted however, that such a resource as this, which enables us to translate a disjunctive into a categorical, can hardly be one of common occurrence. The great majority of the disjunctives which meet us in daily life refer to classes which are disconnected,—often very widely so,—and which common speech has therefore never found occasion to group together under the cover of some common name. For instance, the elements of the disjunction may be two class terms which have little in common, as 'fool or knave': they may be events which are remote in time and disparate in character, as 'be vaccinated, or run a risk of small-pox': they may be individual acts or sayings of distinct persons, as 'quoted from Shelley or from Byron'. In all these cases we *might* invent some common term which should enable us to evade the necessity of a

disjunction; for the resources of language are large, and it is difficult to say of any two things, however apparently unconnected, that we could not discover some common characteristics in them which should serve as the ground of a common name. But in all ordinary circumstances it would be absurd to look for this. The alternative or disjunctive character of the proposition better marks our mental attitude in these cases, namely, that of not merely referring an object to a class, but of recognizing that it belongs to one or other of two subdivisions which are too disparate to be grouped together with convenience.

There is one slight ambiguity which deserves a moment's notice here. It concerns the case in which the subject of the disjunctive predicate is a plurative class as distinguished from an individual. The former admits of one possibility which is excluded from the latter. The difference arises in this way. When I say that, 'All *X* (or some *X*) is either *Y* or *Z*', I do not make it clear whether these *X*'s, all or some, are to lie in one lump, so to say, either in the *Y*-class, or in the *Z*-class, or whether some of them may lie in one and some in the other. From the nature of an individual no such doubt could arise in his case. But such an ambiguity seems to be merely one of the many which common speech has not found it worth the trouble to guard against. If, for instance, I say that, at a certain election, 'All the clergy will give their votes either on High Church or on anti-liquor grounds', I leave it uncertain whether they will all be found to herd on one side only, or to be scattered over both.

So much for affirmative propositions. As regards negatives it seems clear that in their case no true disjunctive is possible; or, rather, it will be admitted that the so-called disjunctive differs here in no essential respect from the non-disjunctive. This arises from the fact that the predicate is necessarily distributed in every negative proposition, so that what we have seen to be the characteristic of the disjunctive,—i.e. its more emphatic, or formal non-distribution of the predicate,—cannot possibly exist here. If, for instance, I say that '*X* is neither *Y* nor *Z*', I simply exclude *X* from the ground of *Y* and *Z* together. So far from increasing the vagueness of the reference of the subject, by the addition of successive alternatives in the disjunction, we actually diminish the vagueness at each such

step. Here, as before, if we can discover a term which just covers the extent of *Y* and *Z*, say *P*, the semblance of any disjunction may at once be got rid of by the substitution of *P* for '*Y* or *Z*'. This holds equally whether *X* is an individual, a whole class, or a part of a class, so that the trifling ambiguity noticed above does not introduce itself into these negative propositions.

There is only one way in which negatives can be introduced into a true disjunction, and that is by the employment of terms with negative prefixes, such as not-*Y*, not-*Z*. But when we say that '*X* is either not-*Y* or not-*Z*', this is clearly in form not a negative but a positive disjunction, and therefore calls for no further discussion.

(2) The other kind of simple disjunction commonly recognized by the logicians, is one in which the *subject* is thus affected, and which may therefore be typified by the form '*A* or *B* is *C*'. The explanations and simplifications offered above do not apply here, and a little consideration will show that the extra uncertainty corresponding to the introduction of this kind of disjunction into a proposition is of a somewhat more serious kind.

The reason why disjunction in the subject is more serious than disjunction in the predicate, is connected with the interpretation of propositions laid down in the last chapter (p. 219). The predicate of the ordinary affirmative is always non-distributed, whilst its subject (if a universal proposition) is distributed. Now we have just seen that the principal change which the introduction of a disjunctive alternative into a predicate which was previously simple—the substitution say of '*X* is *Y* or *Z*', for the simple '*X* is *Y*',—could produce was to *widen the reference*. Hence when, as in the predicate, the reference was already indefinite, owing to the non-distribution, the only resultant change is one of degree. On the other hand when, as in the subject of a universal proposition, the original reference was definite, owing to the distribution, the resultant change may amount to one of kind rather than of degree.

We shall see our way best here by beginning with individual propositions. When I say, 'The Bishop of London will be present', I am referring a definite subject to an indefinite predicate—if the latter is regarded under its class aspect, as it

always may be. Now what change exactly is introduced when for the above I substitute, 'Either the Bishop of London, or the Bishop of Winchester, or so on (enumerating the bishops in order) will be present'? Merely that of substituting an indefinite subject for the former definite one. This is seen more clearly if we replace, as before, the succession of alternatives by one common term which just covers them all. Thus we may say here, 'A member of the Episcopate will be present'; an expression of exactly the same significance to anyone who knows the reference of the terms. The only way in which this differs structurally from the first sentence is that it is a categorical with an indefinite instead of a definite subject.

Now take an ordinary universal affirmative with a distritive subject, such as, 'All clergymen will support the candidate'. What change is introduced here by the substitution of the disjunction, 'Either all the clergy, or all the barristers, or all the doctors will support the candidate'? Here, as before, we happen to have one general name for the aggregate of separate classes in the disjunction, viz. 'the learned professions'; so that we may phrase the statement, without any change of meaning, 'One at least of the learned professions will support him'. Here again the only structural distinction between this sentence and the first consists in the presence of an indefinite as contrasted with a definite subject, for both remain simply categorical.

Now take the case of a particular proposition. Let us suppose that in a certain state of parties, 'the Opposition' comprises Whigs, Radicals, and Home Rulers, and begin with the proposition, 'Some of the Whigs will vote for us'. Take the initial step of substituting a disjunctive subject, so as to change the proposition into, 'Either some of the Whigs, or some of the Radicals, or some of the Home Rulers will vote for us'. Do we here make even that slight structural change which we noticed in the two preceding cases, and which consisted in turning the subject of the proposition from a definite into an indefinite one? Certainly not: all that we do is to slightly increase the indefiniteness which already existed there. We see this more plainly by substituting a single term for the succession of alternatives. Thus compare the proposition 'some of the Whigs will vote', with 'some of the Opposition

will vote'. Both propositions display the indefinite character, with only the slight difference that the latter does it rather more than the former, because the possible range of its subject is wider.

On the whole, then, it seems that we may sum up our present results as follows. All that a disjunction can effect is to widen the reference, by assigning two or more classes for the determination, instead of one only. By this is, of course, meant merely that a reference to '*Y* or *Z*' is more indefinite than a reference to *Y* only or to *Z* only, not that it is more so than reference to *W*, say, by mere virtue of being a disjunction. At this point conventions of language come into play: a mere disjunction, as such, tells us nothing, for if there happen to be a common term just covering the range of *Y* and *Z*, we can, by the substitution of this, get rid of the very semblance of all disjunction.

This concerns what we may call the *quantity*. As regards the *quality*, the question may be phrased thus: Does the change of a proposition from a categorical to a disjunctive make it in any way a different kind of proposition? The answer must be that this depends upon where the disjunction occurs. If it is found in the predicate (in an affirmative) no such change is produced, for the reference of the subject to that predicate was indefinite already. Nor does it do so when occurring in the subject, if the proposition was a particular one: the change of subject from 'some *X*' to 'some *X* or some *Y*' yields no alteration of real character. But if it occurs in the subject of a universal proposition, as by changing 'all *X*' to 'all *X* or *Y*', then since we have rendered indefinite what was before definite, we have made a real alteration in the character of the proposition.

III. So far we have not found any characteristic, either in our ordinary experience or in the propositions which describe it, which seems capable of furnishing a basis for so distinctive a form of speech as the hypothetical. There is, however, another way in which doubt may make itself felt, under circumstances which are more complicated and are such as our experience in the world is constantly requiring us to face; and this it seems to me is just of the kind we require. In saying this it must not be supposed that we are laying it down that the hypo-

thetical form in common speech always and everywhere corresponds to the particular combination of facts and accompanying mental attitude about to be described. Forms of speech of immemorial origin are far too subtle and variable in their development to submit to any such rigid determination of their significance. All that can be claimed is that the form in question probably originated in a certain mental attitude which is one of wide prevalence and peculiar significance; that this attitude still corresponds on the whole to the great majority of cases in which the hypothetical is currently employed; and that where the form is used outside the original or appropriate field of employment, we can almost always trace some link of connection which practically justifies its use. We will begin with a general explanation of this view, and then proceed to the discussion of certain difficulties which are likely to suggest themselves.

The condition of things here referred to is that in which we know that two elements,—events, objects, or what not,—are connected together, but are uncertain about the presence or occurrence, at the time being, of the first member of such connection. It is as if we knew that there were two links of a chain which held together, but were not quite secure in our grasp of the nearest of them. That we should have got to such a point as this represents a certain step in the process of reasoning, for it presupposes some appreciation of the existence of sequences or uniformities; and this, as we shall see, is important as explaining one characteristic of the hypothetical, namely its inferential nature, which has always claimed some notice. In any case such a combination of certainty and uncertainty represents a condition of things sufficiently primitive, widespread, distinctive, and important, to have been able to acquire for itself a peculiar linguistic form in almost all known languages.

As regards these last characteristics there will scarcely be any serious doubt. What, for instance, can have more readily imprinted itself upon the mind of the primitive man than the distinction between those sequences which occur with a regularity and frequency which enable us to foresee them, and those which being equally important and trustworthy when they do occur, yet keep us in doubt whether

or not to expect them? In Egypt seed was sown and the crop gathered with a regularity which seldom showed an exception. But in a country like the north-western parts of India, the attitude of doubt and expectation which correspond to the clause '*if the rain falls*,—then the seed can be sown and the grain will ripen', was one on which the issues of life and death might be involved. Similarly in every department of life. The rising of the sun and consequent daylight is foreseen and taken for granted, and therefore nothing like the statement, 'If there is daylight we will go and hunt', is wanted. But the times of the moon are less apparently regular, and clouds are liable to obscure the light altogether, so that it becomes likely enough that we should require to talk about what we may do 'if the moon shines'. Sequences, or other uniformities, are our guide in life, and our only guide; and therefore as we come to increase our acquaintance with them, we are constantly having to draw a distinction between the cases in which we can appeal to them with prompt and ready confidence, and those in which we have to pause in hesitation, as not knowing whether or not we can make use of them for our purposes.

This particular combination of knowledge and of doubt, though practically widespread as the experience of the human race,—for we can conceive no rational life without a modicum of certainty, and we can foresee no serious prospect of ultimate eradication of all doubt,—must still be regarded speculatively as a temporary or intermediate stage. It requires the aforesaid combination of two elements; and the entire absence of either would destroy the foundation of that attitude of mind of which the hypothetical is the appropriate expression. Take, for illustration, these two conceivable alternatives in turn, and see what would follow when each is pushed to its extreme.

Conceive then, in the first place, a world in which all was uncertain, in which not even a "first vintage" of laws of nature had been gathered in by man. We might then suppose ourselves raising up in the mind one after another of what ought to be first links in a chain, but only to find that there was no second link fastened on to them: the step from the second to the third would be exactly as doubtful as that from the first to the second: all would be coloured with one uniform hue of

utter doubt. In such a state of things it is hard to see how there would be the slightest opening for the hypothetical form of speech: the attitude which it expresses would be lacking. There would be no occasion for prefixing an 'if' to any of the contingencies which we might summon up in fancy, for they would all stand affected with the same entire absence of sequence. Fancy would be everywhere, and reason would be nowhere. If it should be suggested that this is the attitude in which the gambler stands towards the individual results of the die and the card, and that he nevertheless talks of what he will do, 'if' so and so turns up, we may reply that what he has thus in view is the comparatively certain consequent, viz. his own conduct, not the uncertain antecedent, viz. the throw of the die or the draw of the card. A world in which all was doubtful; in which, so to say, all the links of causation were pulverized, and consequently our fragments of experience lay scattered together like a heap of sand, could furnish no ground of foreseen conduct. The hypothetical expression would be lost in the general loss of all power of inference. Consequent and antecedent might of course be called up, by the exercise of the imagination; but there would be none of the regular or inferential passage from one to the other which we now experience.

It may be remarked, in passing, that this is the attitude which should be adopted by the thorough-going conceptualist logicians, if they had the courage of their convictions. Those who maintain that the reality of a concept, for logical purposes, is only limited by its conceivability, and that the logician has no business to trouble himself about the actual truth of any of his premises, have cut themselves off from the recognition of anything in the way of material consequence. They can analyse what is given in a concept, but cannot put any appropriate significance upon such a complex statement as, If *A* is *B*, then *C* is *D*. But of this more in the sequel.

Similarly if, instead of all being uncertain, all were capable of being known. We should link propositions together, and thus proceed from one to another, but the first would stand upon the same footing of confidence as any which followed it. Our knowledge of the world would then have reached that ideal state, which, as was pointed out in the introductory chapters, we have to pos-

tulate as a stage indefinitely far ahead of us, but speculatively possible. Our attainments in the way of knowledge consist at present of scattered fragments of certainty, larger or smaller as the case may be, floating about in an ocean of doubt. In the extreme case contemplated we might describe our experience as having solidified into one huge and connected mass; so that, instead of floundering about until we could reach one of these fragments, in order to take a few steps on what was firm before being immersed again, we could at once set off from any point in order to reach our desired destination. To drop metaphor and take a concrete instance. In our present state of knowledge we now say, 'If the wind goes round to the south the weather will become mild'; because in the midst of our uncertainty we can at least detect a bit of regular sequence. Were all such events wrapped in the same fog of doubt, we should have to say or think, 'The wind may go to the south, and it may become mild'; but there would be no object in putting an '*if*' anywhere into this sentence. Finally, were all known with certainty, we should say, 'The wind will change to the south, and it will become milder'. The first of these clauses would be as certain as the second, and so again there would be no opening for anything in the way of an '*if*'.

(1) We must now look somewhat more closely at the particular form in which the sequences which clothe themselves in the language of hypothesis are apt to present themselves. The reader who is familiar with the common treatment of Logic may perhaps have noticed that propositions of the form, 'If all *A* is *B* then all *C* is *D*', are by no means common. In fact, the form almost invariably adopted in the text-books is that which involves individual propositions, and which presents itself as, 'If *A* is *B* then *C* is *D*'. Now individual propositions being quite exceptional in all other parts of Logic, this peculiarity about the popular hypothetical seems to call for notice.

The reason for this does not seem to be very far to seek. A doubtful general proposition is not at all a common starting-point in practical life. How indeed should it be? To realize a general proposition with sufficient distinctness to be able to trace its consequences, and yet to feel a deliberate doubt about its truth, is common enough when a certain degree of scientific

training has been acquired, but this cannot have been such an easy matter to the primitive men whose intellectual needs stamped the forms of our common speech. To the rude man emphatic affirmatives and negatives compose the bulk of ordinary experience. He has quite enough to do to attend to these, whether they be in reality true or false, without entertaining a further stock which he knows to be doubtful, and which may serve as a starting-point for others, which will of course also be doubtful, owing to their method of attainment.

The starting-point of the hypothetical, or first element of the pair of events constituting the sequence, will therefore generally consist of some kind of individual event; and may in most instances be referred to one or other of the two following classes.

(i) One very common form is that of the application of a general proposition, which is itself known to be true, to an individual case which is felt to be doubtful. I know, say, that all mushrooms are edible, but I have doubts whether the plant before me is a mushroom. I know that a bright, moonlight night will not do for a raid on my neighbour's cattle, but I cannot foresee whether there will be a bright moon to-morrow night. Cases of this sort, in which we are hesitating about the application of such generalizations as we have acquired, whether these be crude and hasty or sound and well-tested, are of perpetual occurrence in the primitive man's daily life, and seem quite capable of originating a peculiar form of proposition. The kind of hypothetical in which they are naturally couched is one of *three* terms, viz. 'If X is Y then it is Z'. This form results from the admission of the generalization 'all Y is Z', combined with the doubt whether X is a case of Y or not.

(ii) In the previous case, as soon as the doubt was removed we had nothing before us but two simple categorical propositions, forming in fact the premises of a syllogism in *Barbara*. 'This is a mushroom: all mushrooms are wholesome':—or in symbols, X is Y, all Y is Z. But there is another very common class of cases of a somewhat more complicated character, which will not so readily fit in with such a simplification. For instance, 'If this witness speaks the truth, then the prisoner had a knife': 'If the seed is sown in March then the corn will ripen in August', and so forth.

Now though, by a certain formal generalization (which we cannot explain here) examples of this kind, as well as that of the former kind, may be expressed by the same simple symbolic form; yet there can be no doubt that this is not the way in which they are most naturally regarded. The propositional form in which they are almost inevitably cast is one involving *four* terms:—'If X is Y then Z is W'. Here X and Y in combination make up a sort of unity, and so do Z and W, but these two unities may be of an entirely disparate character. In the former class of cases our three terms were so closely connected, falling in fact into the materials of a syllogism, that any one of them might conceivably admit of predication of either of the others. But here there is not the slightest reason to suppose that X or Y has such an affinity with Z or W that either of the former could be rationally affirmed or denied of either of the latter[1]. What we really have before us are two events, viz. the fact of X being Y, and that of Z being W, and we are supposed to know that these, *regarded as wholes*, are so connected by a uniformity that, when or where one of them is, then or there will the other be found also.

The fact that we mostly have to adopt a four-fold form of words, that is, one which symbolically calls for four terms in order to express it, in the case of these hypotheticals, seems entirely to depend upon paucity of language. *Thought* recognizes only two elements or unities: the first doubtful as to its present occurrence, the second certainly connected with the first. If we had the requisite words at command, we should adopt a merely dual form, of the type 'If P then Q'. Occasionally indeed we can avail ourselves of this simpler expression, as for example where impersonal verbs will convey our meaning. Thus, 'if it lightens it will thunder' approaches very nearly to this form, and nothing but linguistic propriety prevents us from putting it still shorter and saying, 'If lightning then thunder'. But no difference of signification is indicated by this possibility of abbreviation. It is quite possible that in some languages we might have to build up such a sentence as this in a more

[1] Special attention is directed to this fact because it constitutes the principal difficulty which has been raised against the symbolic rendering which I have proposed for the hypothetical in *Symbolic Logic*.

composite way, as by saying, 'If lightning flashes, thunder will roar', or at still greater length, as by saying, 'If the sky emits flame, the air will give a crash'.

In such cases as this last we are able to reduce, 'If *X* is *Y*, then *Z* is *W*' to the simpler form 'If *P* then *Q*', because the combination of *X* with *Y*, as also that of *Z* with *W*, is so frequent that familiar words have been provided for them in our current language. But of course the vast majority of events, especially events in which individuals play a part, have no appropriate words to designate them, and then we are forced to build up the pair of unities of which the hypothetical essentially consists, by aid of a combination of two pair of terms or sentences. The fact of the prime minister at the time being, say Mr Gladstone, going out of office, is less prevalent than that of the clouds dissolving into falling drops, and has therefore no single impersonal verb or noun corresponding to it; and similarly with the fact of the Queen sending for the leader of the Opposition, say Lord Salisbury. Accordingly, when we want to connect these two facts together, we must resort to a combination of substantives and verbs to express them, and so we get a hypothetical involving the customary four terms:—If Mr Gladstone goes out of office, the Queen will send for Lord Salisbury.

We have implied throughout the above discussion that there is one peculiar and appropriate form of expression set apart for the hypothetical, namely that which commences with the familiar 'if'. But this, though by far the commonest form, has no exclusive propriety for the purpose, and we may conveniently notice some of the alternatives in order to recognize such shades of difference as they may suggest. Take a concrete instance for this purpose. It is observed, say, that a drunken husband causes a squalid home. Presumably any one who wished to indicate this state of things, and who had nothing in view beyond the conventions and proprieties of language, would adopt indifferently any one of the three forms of expression,—'*If* the husband drinks, the home will be squalid', or '*When*', or '*Where* the husband drinks, &c.'

The general state of things thus indicated by these various phrases seems to fall within the limits we have assigned to the use of the hypothetical; for there is a connection asserted

between the antecedent and the consequent,—the drunkenness and the squalor,—and a doubt implied about its occurrence in this or that case But of course, when we look closer, we find that, as in almost all employment of synonymes, there are shades of difference recognized which decide the suitability of the particular phrase selected. The particle 'if' is specially suitable when we have an individual case in view,—as though, for instance, we went into some house where we had suspicions as to the character of the occupant. We generally confine ourselves to this particle when the event is one which is necessarily unique; thus I should have said, 'If Lord Nelson is killed in the next battle Admiral Collingwood will take his place', and we clearly could not here substitute 'when' or 'where' for 'if' in this sentence. The class of cases in which this substitution is admissible seems to me to be that in which something is known, by an approximate generalization, to happen in a number of instances, but where, when an example is taken at random, we cannot say whether or not the phenomenon will be found to occur there.

Thus in our example above, about the drunken husband, it will hardly be disputed that either of the above three forms of expression would be equally natural and appropriate, or that the structure of the sentence and the point of view under which the matter is regarded is fundamentally the same. In each case alike we are doubtful whether some undetermined individual falls under the observed generalization. The only difference seems to be that in the one case the uncertainty is just indicated, and nothing more, by the use of an 'if'; whereas by the employment of the other particles we are reminded that there are plenty of cases in which the generalization does not apply, and that therefore if our doubt is to be removed we must select the applicable cases by choosing the place *where*, or the time *when*, the suitable conditions will be found.

(2) Having thus discussed the essential characteristics of the hypothetical the next question which demands consideration is the closely allied one as to the constancy and unanimity of popular usage in its employment. As a matter of fact, do the categorical and hypothetical expressions keep clear of one another, each on its own appropriate ground? In answering this question we must insist again upon a point which has been

noticed more than once already. Logicians have been too much in the habit of attempting to decide what *is* the meaning of such and such a form of expression, as if it really retained a fixed meaning throughout. But language is much too mobile a medium to consent to submit to such constraint. We may assign its original channel to the stream, but we shall generally find that the current proceeds to shift its banks; sometimes overflowing them, sometimes contracting itself within them, sometimes seeking a new course outside the old one. That is, dropping metaphor, we may find the hypothetical form of speech adopted for one reason or another in cases which appropriately belong to the categorical, and conversely.

Stripped of all grammatical variations and embellishments, the categorical and hypothetical may respectively be reduced to the forms; '*X* is *Y*', and 'if *X* then *Y*': *X* and *Y* being called the subject and predicate in the former case, and the antecedent and consequent in the latter. The original and fundamental distinction between these forms I hold to be, as has now been fully explained, that the presence or existence of the *subject* (in the categorical) is taken for granted, whilst that of the *antecedent* (in the hypothetical) is recognized as being doubtful. It follows therefore that the question now before us may be subdivided under two heads:—(i) Is the categorical form ever adopted where the subject is doubtful? (ii) Is the hypothetical ever adopted where the antecedent is certain? Both these questions will have to be answered with a qualified affirmative, thus indicating a certain laxity in the retention of the original distinctions. This must be followed out in some detail.

(i) The former question amounts to this. When we utter a categorical proposition, is the existence of its subject,—existence in the wide sense indicated in the introductory chapter,—taken for granted? This is a topic which has been strangely neglected amongst logicians, hardly any English writer before De Morgan having even recognized it as a possible enquiry, and some since him having scouted it as irrelevant to Logic. I have discussed it (*Symbolic Logic*) pretty fully elsewhere, and Dr Keynes (*Formal Logic*) has given a careful analysis of the various bearings of the problem, coming to the same general result as my own.

The main conclusions seem to be as follows. Common convention almost always takes it for granted that the subject of the universal affirmative, as of the negative, is something which actually exists, and always takes it for granted that this is so with the subject of particular propositions. And it seems only natural that this should be so; that is, that our positive assertions and denials should refer to things which we know do actually present themselves within the range of appropriate experience. In so far as the difference between the universal and the particular proposition is concerned, it seems to me probable that the superior certainty of the subjects of the latter is to be found in the fact that whereas universal propositions are often derived from general reasoning, from tradition or authority, or even from mere presumption, the particular must, from its nature, far more often rest upon observed instances. When we predicate of 'some' only, it is often because we have observed those very instances, so that no doubt as to the occurrence of the subject can possibly arise.

The point is one which must be decided by the reader for himself, since technical authority has pronounced no decision here. But in deciding it he must be careful to keep in mind a distinction which Common Logic sedulously endeavours to erase. One of the first cautions impressed upon the beginner is the insignificance of the distinction between terms composed of a single word and those which are built up by several words. Our X and our Y have no better warrant, we are quite correctly given to understand, to denote a 'horse' than to denote the 'persons who lead a black horse down Piccadilly on a Saturday afternoon'. And it is right to insist upon this,—as was done in the introductory chapter,—because in all cases alike the unity attributed to the subject is obtained by the same sort of mental synthesis whether the words employed to denote it be few or many.

For our present purposes however the distinction in question must be revived, as being of real significance in the interpretation of hypotheticals. Its importance rests on the following ground. Things denoted by single terms generally have an existence past all dispute or doubt. The primitive man has something more pressing to do with his vocal organs and inventive powers than to impose names upon objects which he

merely contemplates as possible. In fact it is only the objects of most paramount importance, or greatest frequency of occurrence, which are likely to get a chance of being named at all. Of course in saying this it is not suggested that the objects thus stamped with the warrant of a single name must be still believed in by later generations. Language is far too conservative for this. A name originally imposed upon objects supposed to be quite accessible to common experience may now indicate an existence as shadowy in its outline as a chimera or as universally rejected as a mermaid. But making every allowance of this kind the fact remains that there is a very strong presumption that any subject of a sentence which is designated by a single term can vindicate its existence or actual occurrence.

Very different is it with a many-worded subject. Its existence cannot be so readily taken for granted, for it has passed through no such test of ancient usage. Accordingly we shall not find convention so stringent in its support; on the contrary it is felt that its actual occurrence must be justified by specific experience. If we look at the way in which propositions with these complex subjects are actually treated we shall find that they hold a very different place in popular estimation. In fact some doubt is often felt as to whether they should be retained in the categorical form. As a rule popular usage abandons the strict categorical form whenever the existence of the objects denoted is seriously doubted. Sometimes we adopt the hypothetical rendering: sometimes we incline towards a compromise with this, selecting some such form as that of which, 'the *A* that is *B* is *C*', may be regarded as one type. This rather happily indicates that though *A* and *B* separately are notoriously recognized as existent yet that their combination together is thought doubtful.

Propositions of this latter type remind us how slender is the partition which divides the categorical form from the hypothetical, and how difficult it is to retain the distinction when we advance beyond elementary propositions. The common logician is puzzled how to treat them, though it must be admitted that he has often settled the matter by avoiding them altogether. Those who have accepted them have generally classed them under the head of limitative or exceptive propositions, on the ground that they only attribute the subject to the

predicate under a limitation, that is, under a condition. To me it seems plain that they belong essentially to the hypothetical class, for the simplest form to which they can be reduced is 'X (if there be any) is Y', where X represents the uncertain conjunction of two separately certain elements, and is therefore itself in consequence uncertain.

The general conclusion from the foregoing observations seems to be as follows. When we make a wide examination of propositions, so as to include those which possess complicated subjects as well as those whose subjects are simple, we find it very difficult to keep what may be called the purity of their categorical character intact throughout. Propositions with simple subjects are adequately expressed by 'X is Y', with the accompanying implication that X does really occur: propositions with complicated subjects, on the other hand, can only be interpreted to mean 'X (if there is any) is Y'.

Under these circumstances the logician, to whom consistency is always an important duty, is bound to point out how he proposes to meet the difficulty. Two principal courses seem to be open before him.

(a) One plan for securing consistency is to interpret *all* categoricals as meaning no more than 'X (if there is any) is Y'. This has actually been proposed by some writers (e.g. Spalding), but as it seems to me with a very inadequate appreciation of the consequences of their proposal.

That there is a certain treatment of Logic,—namely that which may be called Symbolic Logic,—in which this rather extreme course has to be adopted is quite true. But such an assumption is here a part of a somewhat wide scheme. We start with the postulate that no term whatever indicates the existence of any object corresponding to it, thus abrogating all distinction between categorical and hypothetical. From this postulate, which in itself represents a considerable departure from popular convention, still more serious consequences follow: for instance, we are forced to interpret all universal propositions simply by what they deny. For a full discussion of these and other consequences the reader must be referred to what I have elsewhere said upon the subject. At present all that need be said is that this wide departure from the ordinary view is forced upon us by the necessity of grappling with propositions of every degree of

complexity. The simple propositions prevalent in the ordinary syllogism demand little else than the conventions of common speech to help them through. But when we come to deal, not merely with one, but with half a dozen propositions of which a simple specimen might be 'If any XY is Z then it is either V or W', we soon find that the attempt to work under any other assumption than that just mentioned leads to inextricable confusion.

But to adopt this assumption, with all its consequences, in a very special treatment of Logic, is quite a different thing from admitting it into the ordinary treatment. The common system does best in adhering as closely as possible to the conventional modes of thought; and since these do as a general rule take it for granted that wherever there is a subject-term employed in a proposition there must be some actual objects corresponding to it, the rules of Logic should as far as possible be kept in harmony with this convention.

(b) The other available plan is to retain the popular convention intact; and accordingly, when we find any proposition with a doubtful subject, to treat it and express it as a hypothetical. If it be asked what can be the objection to doing this, and why it should need so formal a statement, the answer is that logicians are apt to find their feet entangled here in a net of their own contriving, which they are bound to respect, but which plain people have little scruple in kicking to pieces. This consists in the rules they have laid down as to conversion, contraposition and so on, or rather in their claim of unconditional applicability of these rules.

We can only give a brief intimation of the difficulty here[1]. We start with a simple 'all X is Y' which we consider to guarantee the existence of X. But, when we convert this, Y becomes the subject and the same claim is at once raised on its behalf. Then we proceed to contraposit and convert, and are accordingly led on to similar admissions as to not-X and not-Y. In other words we are gradually brought to admit not only the existence of X and Y, but also of not-X and not-Y; i.e. that neither X nor Y is all-embracive within the sphere with which we have to deal. This is vastly more than the plain man ever thought of admitting when he started with his X.

[1] For a very full and clear discussion of it see Dr Keynes's *Formal Logic*.

Common sense has of course a ready mode of getting out of these difficulties. It simply does not recognize the right of indiscriminately carrying out these processes of conversion and contraposition: or rather, where it does admit them, it explicitly recognizes the hypothetical character of the resultant proposition. Take a concrete example:—'No woman was burnt for witchcraft in the reign of Anne'. This is a case of 'No *X* is *Y*', from which the logician's 'No *Y* is *X*' would follow at once in the shape of 'None of those burnt for witchcraft in the reign of Anne were women'. But it is quite certain that any unsophisticated person would demur to the proposition when couched in the latter form. He would either insist upon expressly introducing the saving clause 'if there were any such', or he would explain that this clause was to be taken for granted. That is, he would not admit the free right of conversion as the logician claims it.

Perhaps this would be, on the whole, the best course to adopt; but the Common Logic does not seem to have made up its mind on the subject, or indeed, till lately, to have seriously faced the question. If we so decide, we should recognize all the ordinary rules for conversion, contraposition and so forth; but we must be prepared to admit that the propositions which result from their employment may (under conditions and explanations which it would be out of place further to discuss here) really be hypothetical instead of categorical.

(ii) So much for the first of these enquiries as to the possibility of retaining a clear line of demarcation between the two forms of proposition. The second raised the question whether propositions hypothetical in form are ever categorical in their real character. This must also be answered with a qualified affirmative, since common speech often shows a disposition to couch in hypothetical form statements which are not truly hypothetical in their character. Even in these cases, however, we can generally find some trace of the characteristic quality of this grammatical form.

There is, for instance, what has been called the 'hypothesis of inference', in order to distinguish it from that of doubt. Of this an example has been proposed in the remark of Col. Morden in *Clarissa Harlowe*: "If you have the regard for my cousin which you say you have, you must admit, &c."

Lovelace fires up at the assumed insinuation, on which the colonel replies that his 'if' prefaced a conclusion and did not necessarily suggest a doubt. This seems a rather lame explanation. When, as here, we are concerned with the current signification of language, the fact that offence should be taken seems tolerably conclusive. It appears to me, as was said at the outset of this discussion, that there is always inference conveyed or intimated in the hypothetical form. The utmost we can admit is that whereas in normal cases this inferential element is only one of two equally important characteristics, it may occasionally become the dominant element owing to the other, viz. the doubtfulness of the premise, having sunk into insignificance. It is difficult to believe that popular speech would tolerate an 'if' where the antecedent is regarded as really certain: the utmost length to which it is inclined to go in this direction is that of simply agreeing to set aside the truth of the antecedent so as to lay the stress upon the following of the consequent. Had Col. Morden really felt no doubt about the existence of the regard he would surely have prefaced his sentence with a 'since' instead of an 'if'.

Another class of apparent exceptions may be illustrated from constructive geometry, where we constantly find propositions couched in the form, 'If a straight line be drawn cutting two parallel straight lines, the angles on the same side shall be equal'. Here, it may be urged, there is no shadow of doubt about the occurrence of the antecedent. The answer seems to be much the same as in the last case; viz. that though the inferential element has become predominant, that of doubt has not really disappeared. The fact is that we have recurred here to the class of examples discussed a few pages back, where it was pointed out that the hypothetical form might be appropriately used to cover the uncertain identification of an individual, provided certain consequences followed on his recognition. We saw that such a statement as, 'If the husband be a drunkard the home is a wretched one', simply indicated that any husband taken at random might or might not fit the designation, but that drunkenness was pretty constantly followed by such misery. And we saw that such particles as 'when' or 'where' might be substituted for the

more usual 'if', with but the slightest change of signification.

So it seems to be in geometry. The straightness of the lines and their parallelism are necessarily connected with the equality of the angles alluded to, but there is no necessity that these characteristics should be present at any given time or place. Still less is there any necessity that we should proceed actually to draw such a line. Here, as before, we may equally substitute 'when' or 'where' for 'if'. The sort of thing postulated when these geometrical propositions are advanced is, I apprehend, that of the learner with his ruler, compass, and paper before him. He may draw what straight lines and angles he pleases, so it is uncertain what we may find drawn in any particular case. We are therefore perfectly in order in employing the hypothetical form and saying that *if* he draws such and such figures, such and such properties will necessarily be found to be involved in them.

The general conclusion therefore which seems to follow as to the distinctiveness, and the propriety of application, of these forms of speech is this. The distinction of facts, or rather of our mental attitude in relation to those facts, out of which the structural difference between the categorical and hypothetical forms of assertion has sprung, is a real and important one. So soon as men had begun to observe and to reason, this distinction must have forced itself into notice in every department of practical and speculative life. And the distinction thus early recognized has been imprinted upon our forms of speech, and has thus been enabled to establish and perpetuate itself. But, as in all other cases where language is concerned, the forms of speech have shown a disposition to modify themselves with lapse of time and growing complexity of circumstance, and have thus become somewhat shifted from their original application. The hypothetical, it is true, as commonly employed, has lost but very little of its primary import: that is, some trace of the conditions under which it may be supposed to have originally developed itself may still be detected wherever it is employed. But the categorical form has shown a more decided tendency to extend its scope over what is really hypothetical ground. It has been driven into this course by the gradually increasing complication of the subjects of the propositions which advancing thought

demands; for, as we have seen, complicated subject-terms cannot be considered to be bound by the assumption,—namely that of the actual existence of objects corresponding to these terms,—in virtue of which alone categorical propositions can be technically distinguished from hypothetical. This tendency finds its extreme development in the Symbolic Logic, in the usual treatment of which all distinction between the hypothetical and categorical forms is entirely obliterated.

CHAPTER XI.

DEFINITION.

WE have now reached a part of the subject in which the old technical terms have so worked their way, not only into the current language and treatment of the modern logicians, but even into the phraseology of common life, that a somewhat more historic explanation than has hitherto been adopted seems advisable. In treating of Definition we find ourselves forced to discuss the famous Predicables, or Five Words, which from the time of Porphyry onwards long formed the central part of Logic as commonly treated, and to the consideration of which indeed many entire treatises have been devoted. This however introduces us to a certain difficulty. The old technical terms are too thoroughly established to be lightly abandoned. But intimately connected as they are with decaying and obsolete doctrines, it is impossible not to make alterations in their meaning, and the extent to which this has been done by various modern writers has introduced an element of variety and confusion. Some of the more conservative writers have clung so closely to the old ways of thinking that we find them still taking for granted the scholastic distinction between necessary and contingent matter. Others, though retaining the old terms, have endeavoured to translate them into purely modern ideas, and have naturally found it very difficult to find a suitable and consistent usage for them all.

The following is the plan which will be adopted here. We will first explain what may be regarded as the customary modern usage of sound and sober logicians. A brief sketch must then be given of the old doctrine, in order to realize how wide is the

change in meaning and association which is bridged over by the retention of the ancient phraseology. So much refers mainly to the province of Formal Logic. We will, afterwards, consider whether any of the old terms are still capable, and if so in what application and under what limitations, of answering the past and present wants of the Inductive logician.

I. The "five words" with which we have to deal, it need hardly be said, are Genus, Species, Property, Difference, and Accident.

(i, ii) First then, as regards the Genus and Species, which it will be best to take together. These are now commonly regarded as class names; or rather, to speak more accurately, as the classes themselves denoted by the names. And no further or deeper distinction is recognized between the two than that of greater and less denotation. That is, whenever two classes of things are found, with names corresponding to them, of which one includes the other, they may be respectively so regarded: the wider being considered a genus, and the narrower a species.

In saying this it must be understood that when we talk of the wider class including the narrower, we mean that it does so formally: that is, that the connotation of the latter includes that of the former. It would not generally be considered correct to select any two classes of which one happened to lie inside the other, and call the former a species of the latter. Thus, even if all kangaroos were as a matter of fact natives of Australia, the kangaroo would not be technically regarded by the logician,—and still less by the naturalist,—as a species of Australian thing, because its residence or place of origin is in no sense a part of the meaning of its name. But for the same reason it would rightly be regarded as a species of marsupial, of mammal, and of animal, because its distinctive attributes include those of each of these classes.

(iii) The account generally given of the Differentia follows simply and immediately from that of the Genus and Species. The species, as we have seen, is a narrower class selected from the broader by the addition of some further attribute or attributes. The surplus connotation thus involved in the species over the genus is regarded as the Differentia of the former.

(iv) So far there is but little opening for variety or difficulty amongst those who only seek a reasonable and consistent usage of the old terms. The three words referred to, or at least the distinctions which they are intended to express, are absolutely requisite for accurate discussion. Indeed, we might say that even the looser discourse of common life could not be conveniently carried on without some resort to them. The next term however, *Property* or *Proprium*, seems of less importance, and its retention of a place amongst the others is mainly due to tradition. It is moreover that one of the five words as to which the widest difference of interpretation is to be found amongst the old logicians. If it is still to be retained, probably the best interpretation is that of Mill and some others. On this view the 'property' is regarded as being any attribute which does not explicitly form part of the connotation of the term in question, but which can be shown to follow from what is a part of such connotation. The reader may be supposed to know,—it is a point to which we shall have to recur presently,—that the logical connotation of any term comprises a limited number only of those attributes which the objects themselves comprising that class actually possess in common. Of the remaining attributes some will probably be deducible from others. If so, and these latter are themselves included in the connotation, then those which are deducible will be considered as properties. Thus, for instance, the Differentia of a Bill of Exchange consists mainly in the fact that it only becomes due after a certain assigned date. That is, this is the principal distinguishing attribute which differentiates it from other instruments of credit, such as cheques and banknotes. Now it is a consequence of this characteristic that Bills of Exchange will be more liable to fluctuations in value than obligations which are payable at sight. Accordingly the fluctuation of value may be regarded as a 'property' of a Bill of Exchange. It is no part strictly of the meaning, but follows from that which is a part.

(v) Accident. Here again we come upon a term indicating a generally recognized distinction, and one consequently which has worked its way into popular phraseology. On the whole it is still accepted in a sense which departs but slightly from its original signification.

Every class of objects, as just remarked, contains many attributes, besides those connoted by the name, which are common to every member of the class. And every individual object contains an infinite number of attributes which are in no way involved in the meaning of any general name which we may happen to apply to it. These attributes may differ widely in respect of their permanence and prevalence. Some may only just have fallen short of being included in the connotation, on the ground that though not included in the meaning of the term they are always present, and are inevitably suggested by the use of the term. Others may be very frequently present. And others again may be of the most casual character, mere temporary qualifications, or so forth. But they all agree in the fact that they cannot strictly be inferred from anything contained in the connotation of the name. These attributes are called *accidents*. They fall naturally into two classes. Sometimes they will, as a matter of fact, be found to be present in all the objects of the class in question: they are then called 'inseparable' accidents. Sometimes, and of course much more often, they will only be found present in certain individuals of the class, or only present at certain times or under certain conditions. They are then called 'separable' accidents.

Thus among the inseparable accidents of the Bill of Exchange might be included the facts that it is in great part printed or lithographed, and that it is on paper: among its separable accidents may be included the size, date, value, &c., in fact, all the innumerable qualities by which one bill is distinguished from another.

II. Having cleared the ground by the foregoing brief discussion we are now in a position to say what is to be meant by *Definition*. Revert for a moment to the distinction between denotation and connotation. We have already seen the necessity of assuming that every significant name (with some easily assignable exceptions) has a certain number of attributes commonly understood to be implied by its use. That is, we must suppose that there is a substantial agreement amongst competent and correct speakers as to what is involved in the meaning of the name. The aggregate of the attributes so involved constitutes the connotation. This aggregate forms

a sort of central nucleus, around which are grouped an indefinite number of others, some of which are always present, some only occasionally. These latter constitute the properties and accidents of the objects. The Definition of any name is simply the enumeration of the component items of its Connotation.

Nothing can be simpler therefore than the mere statement of what is understood by Definition on this view. There are perplexities enough awaiting us when we come to apply the process,—as we shall almost immediately see,—but the aim we set before us is really nothing else than that of just stating the attributes which it is understood that we all of us 'mean to imply' when we use the word.

The first thing to notice about this account is its departure from the old view. The wide range of this departure is best indicated by the change of signification which it requires us to impose upon a certain word,—namely, *essence,*—which we have not yet had occasion to employ. It would demand a whole chapter if we were to attempt to give a historic summary of the various shades of meaning which this word has borne: but one or two main points may be stated without difficulty.

The characteristic element in the meaning of this word 'essence' is necessity or indispensableness. This meaning it has retained unchanged, but the application has varied through the whole range from the objective to the subjective; namely, from a presumed necessity imposed upon things by the laws of nature, to a conventional necessity imposed upon us by the usages of language.

The old view has been summed up by a recent supporter, Hamilton, in the statement that the essence comprises those qualities without which the thing 'would cease to be': the modern view would put it that the essence comprises those qualities without which we should not apply the name. It might seem at first sight as if the difference between these statements would not amount to much, on the ground that the only rational scheme of imposing names is to follow as closely as possible the qualities which we find to be possessed by the things which we name. There is, however, a very serious difference between the two ways of looking at the matter. An obvious objection to the former mode of expression is that it

launches us into a sea may 'fficulty and ambiguity as to the nature of existence, when it is perfectly unnecessary, for any logical purposes, to start in that direction. It suggests difficulties and incompatibilities on the part of nature which often only apply to our task of imposing names. Take a familiar instance. A knife consists of blade and handle. Remove either of these and something perfectly substantial is left behind, but it certainly is not what we call a knife. So with a lump of ice. Melt it down or vaporize it and exactly the same mass of matter will remain, and in the same chemical, and presumably similar atomic condition; but we no longer call it ice. In these cases it is plain enough that there is no kind of difficulty on the part of the objective facts; the strain is only felt when we attempt to express the facts without changing the name.

Keep clearly before the mind the distinction between the name and the thing named,—a distinction, some of the consequences of which were drawn out in the first chapter,—and we see that it is likely to breed confusion when we insist upon saying that the ice or the knife has 'ceased to be'. Of course no serious consequences follow in examples of such a familiar character as this, where all the relevant facts are clearly understood. The mischief is done where we are dealing with more recondite subjects. It need hardly be said that we are not proposing that the common expression should be banished from common discourse. To insist that we are not to say, for instance, of the condition of a country in an extreme state of disorder, that 'government no longer exists there', but are always to phrase it that 'the name of government is no longer applicable to the state of things there', would be sheer pedantry. We ought however to recognize that the two expressions are equivalent, and that the latter is really the more accurate in itself. It is impossible for us to be consistently nominalistic in our current speech, but we ought from time to time, and especially in our formal definitions, to make it plain to ourselves and to others what we really mean[1].

[1] Tradition is still powerful in guiding our verbal usage here. Few persons, for instance, who had occasion to use the term 'connotation' would apply it otherwise than to the *name*: they would naturally speak of 'the connotation of the word government'. On the other hand, if they use the word 'essence' they

The main source of difficulty, [illegible] when we come to apply our account of Definition to words actually in current use, seems to lie in the fact that the strict 'meaning' of most words is apt to display anything but a clear boundary line. The general fact was fully admitted and described in the Introductory Chapter; the difficulty meets us here when we come to decide what exactly is implication and what is merely strong association. It meets us equally, we must remember, in the case of proper names and in those which are really general. The former should, of course, have *no* 'meaning' or implication; but we may easily see how strong are the associations which they can excite, and how much information will commonly be gathered from them. Begin with one of these, which we have already mentioned in a former chapter. If I were to see such a name as 'Christopher Jenkinson Simpkins', I should feel an unhesitating conviction that it denoted a male human being: an only somewhat slighter conviction that he had a father whose name was Simpkins; and a presumption, quite strong enough to give a hint in my search, if I were enquiring as a genealogist, that there had been a relation of his parents whose name was Jenkinson. Now every particle of this information has been elicited from the mere name itself: what then is meant by saying that the name has no meaning? All that we can here reply is that a line has to be drawn, and drawn as it always must be done, by the aid of experience and common sense; and that when this line is traced it seems that such instances as this lie just outside it. That is to say, such a name as the above might be given without actual error to something else than a man, say to a house or a ship: also, however customary it may be to confine 'Christopher' to males, there is no obligation to do so: and, in fact, Christian names are sometimes deliberately thus misused. The distinction here insisted on is really nothing more than the universally recognized distinction between presumption, however violent, and inference. There is nothing actually *wrong* in calling a girl 'Christopher' as there would be in saying she was a boy: Simpkins will grievously mislead the genealogist if he calls his son by the surname of Tomkins, but he has not told a falsehood in so doing. In a word;

would almost inevitably apply it to the thing: they would speak of 'the essence of government'.

the task, though it may be a difficult one, is not hopeless, when we endeavour to make out what a name actually means or implies, and what it merely suggests or gives a presumption of[1].

What thus holds of proper names, which have no true meaning, holds equally of general names which have a meaning. Around the central group of essential attributes there is always found clustered another group composed of those which just fall short of being essential. Their importance will be very variously estimated by different persons,—of this, and its consequences, we shall have more to say presently, when we come to discuss the causes which lead to the change or abandonment of definitions,—but we can at least address ourselves to the task, as to a rational one, of saying which are the attributes really involved in the meaning. And in the last resort, if two or more of us differ hopelessly in our decision on this point, then we ought each to be able to say what we respectively mean by the word, and what we consider that it does no more than suggest to us.

III. *Nominal and real Definition.* We have already made a slight reference, when touching on the nature and interpretation of the 'Essence' of a thing, to the distinction between the object and its name. There is however a somewhat different and far more familiar aspect of this same distinction forced upon our notice in the well-known traditional distinction between the Real and the Nominal Definition, which we must now proceed to discuss. The distinction is an old one, and has shown much tenacity in retaining its place in modern logical treatises, though it has had to undergo a wide variety of interpretation.

(1) The account which Hamilton gives is as follows. "By verbal definition is meant the more accurate determination of the signification of a *word*: by real, the more accurate determination of the contents of a notion. The one clears up the

[1] A consequence from this deserves notice. The essence of any individual object is entirely determined by the name through which we regard it. Point to an object and ask, What is its essence? We cannot say, for until it is named the question is entirely indeterminate. But give it a name,—any one of the innumerable common names which can be selected as applicable to it,—and the question becomes determinate at once, for every such name carries its connotation with it.

relation of *words* to *notions*; the other of *notions* to *things*."[1] We surely cannot attach much value to this account, for what else is the 'signification of a word' than the 'contents of the notion' corresponding to it? the two seem to me to be precisely equivalent. To attempt therefore to draw a distinction between the notion and the word in this way seems to me to be idle. The really important distinction here is that between these two taken together on the one hand, and the object denoted on the other; for the latter, namely the object, possesses innumerable attributes, whilst each of the former contains a narrowly limited selection from amongst these. But between the word and the notion what difference can there be, as regards their content or reference? The word, though of course ultimately referring to the object, is at first hand nothing but the spoken or written symbol for the notion. Whatever necessary attributes the notion contains, those same attributes, neither more nor less, must the word imply. Both may have various faults or defects as regards their relation to the object, but these faults or defects they must possess in common.

(2) Other writers who retain the distinction have offered a different explanation of it. They hold that the nominal definition analyzes the notion, and does not attempt to go deeper; whereas the real definition reverts, so to say, to the true source of all definitions, and by examining the object itself is prepared to amend the definition where necessary, or add to it. In so far as this account retains the close connection between the word and the notion,—a connection which Logic must regard as indissoluble,—it seems to me correct. Moreover, it must be admitted that the process of thus referring back to our authorities is a highly desirable one. Unless our knowledge,—not merely in regard to propositions, but also in regard to notions or terms,—is from time to time tested by reference to experience, it will soon begin to deteriorate. But ought this process to be regarded as part of a Definition? Surely not. It is a process the adequate performance of which is presupposed in every definition and in every branch of science. We can say therefore that some definitions are

[1] Hamilton's Edition of Reid's Works, p. 691. This is a later and clearer account than that given in his Lectures on Logic.

better than others, because the requisite guarantees have been more fully secured; but the more or less complete attainment of an end which is aimed at everywhere, cannot be the ground of recognizing two distinct *kinds* of definition as indicated by two such distinct names as 'real' and 'nominal'.

(3) A third, and very different explanation has been offered by Mill, and has obtained the wide acceptance due to his authority. On this view both the real and the nominal definitions are to be regarded as definitions of names only; but there is a broad distinction between the implications which they respectively carry with them; in that the former does, and the latter does not, imply the actual existence of objects corresponding to the name to be defined.

The distinction thus laid down by Mill is the outcome of an interesting and important discussion as to the function of definitions, in which he shows,—what is indeed obvious enough when pointed out, though it had been much overlooked or transgressed,—that from a mere definition nothing can possibly be deduced except conventions of language: wherever anything more seems to follow from such an origin it is really derived from an implied assumption that there are such things in existence as those which we define. He illustrates by the instance of the so-called genetic definitions of geometry, and by definitions of recognized non-existences (according to any physical test), as in the instance of *dragons.*

This question of the 'existence' of the various objects which we name has already come under our notice more than once. Where Mill seems to go wrong here is in the assumption of a hard and fast line between existence and non-existence; as though this distinction could only be accepted in one sense. That the student of physical science can only tolerate one signification of 'truth' is certain; and the predominantly physical cast of his Logic, and his aversion to mere formality, have probably induced Mill to recognize no other signification than this. But Logic has a wider application than is recognized by the physicist, and we must therefore widen, in a proportionate degree, our interpretation of 'truth' or 'existence'. To say therefore that certain definitions imply the existence of corresponding objects is insufficient, unless we also indicate

in which of the various possible senses we intend that term to be understood.

Probably the wisest thing to do is to rid Logic altogether of this distinction between real and nominal definition. The best general account of the matter would then be this. All language, as a broad *primâ facie* presumption, carries with it the implication that the speaker believes in the reality,—that is the physical reality,—of the things corresponding to the words which he uses. People do not speak with an intention to mislead, nor do ordinary adults talk habitually of non-entities[1]. The mere use of a word therefore raises the presumption of the phenomenal reality of the objects answering to the word. This presumption is the general rule, but marked exceptions have to be made in one or two directions. For one thing there are the writers of fiction, the tellers of fairy tales, the narratives of inventive children, and so forth, where no pretence is made of aiming at fact or even, it may be, of preserving consistency. And again, there are sciences, as we may term them, such as Heraldry, and certain kinds of ancient or sacred art, where adherence to more or less arbitrary convention takes the place of the agreement otherwise attained by continued appeal to experience. In these cases accurate definitions are possible, and should be secured if convenient, but the employment of them is clearly subject to the well-recognized character of the matter in question.

We may say then that every definition, with such exceptions as those just made, raises a presumption of the physical reality of the objects to which it refers. But it certainly ought not to claim more than to raise such a presumption; unless of course reality is formally incorporated into it. Nor indeed is it easy to see how a definition could intimate such a claim. If some definitions do, and some do not, carry reality in their train, how are we to ascertain to which of these classes any particular example may happen to pertain? There is nothing on the face of it which could decide the question, and we

[1] "Right sure am I, Sir George Mackenzie says, that no divine can doubt there are witches, since the Bible says thou shalt not suffer them to live; and that no lawyer in Scotland can doubt it, since it is punishable with death by our law. So there's baith law and gospel for it." (*Waverley*, ch. 67.)

should accordingly have to fall back upon some such general presumption as that just indicated.

It is best therefore to reject this distinction altogether. If any one, however, who wishes to retain it asks to which of the two classes definitions, as we here understand them, are to be considered to belong, we reply unquestionably to that of the Nominal. A definition is merely the interpretation of a name. In and by itself it has no warrant to convey one kind of reality rather than another, nor has it any known means of doing so. It stands in fact on precisely the same footing in this respect as a term or name. If one of these is uttered we have to judge, by the context or the subject-matter, to what order of existences it belongs, and we must do the same in the case of definitions.

IV. The next question for discussion concerns the *limits* of Definition. In other words, over what range of existences of any kind can we reasonably ask for definitions, and where and why must we cease to do so? The answer to this enquiry turns in great part upon the kind of definition we propose to offer.

(1) On the old scholastic view the limits assigned were quite definite. Every class, except the widest, must be included in some genus, and be marked off from it by a differentia, and must therefore possess the elements of a complete definition. The point needing explanation here, however, is as to what must be reckoned as the widest class. Some writers speak as if this must always have been held to be *Being* in general. This however was the view of none, or next to none, of the Aristotelian logicians. They took the Categories as their standard, and looked no further upwards than to the highest class in a Category. These ten Categories were regarded as so radically distinct from each other, that it was a misapplication of the process of abstraction to attempt to bring them under one single head. Accordingly the upward limit of definition in each category was reached at the highest class but one in that category. In the other direction the limit was reached when we got down to an *infima species*; that is, one in which the members were separated by no essential, but only by accidental characteristics.

One other exception must also be noticed. These Cate-

gories were by no means intended, as sometimes stated, to be a 'list of all nameable things'. On the contrary there were a number of things which were definitely excluded from any category, and which were consequently incapable of technical definition. They were generally summed up as follows:—

> "Complexum, Consignificans, Privatio, Fictum,
> Pars, Deus, Æquivocum, Transcendens, Ens Rationis,
> Sunt exclusa decem classibus ista novem."

(Sanderson, *Compend. Log.* Another version is given by Seton in his *Dialectica.*)

(2) The modern popular view is of a very different kind. Starting with the postulate that every name which has a meaning must offer occasion to have that meaning unfolded, it is rather apt to extend the 'meaning' so as to make it cover correctness of application in general. Thus the names of simple sensations, which strictly speaking possess denotation only, may yet according to some writers yield a kind of definition, for we may analyze their physical antecedents or accompaniments. This view dates presumably from Locke[1], whose attitude however towards formal Logic is not one which we can safely imitate.

(3) The view noticed above shows a not unamiable bias towards making Logic useful, or rather towards converting it into a body of useful rules, for the improvement of language. My own opinion is that we do best to admit frankly that Logic is only concerned with the 'meaning' in the strictest technical sense of the term, that is, with the connotation of the term. There are many ways of guiding any one to a right use of words, and of these the comparatively artificial method, of analyzing the term by assigning the component elements of its connotation, is the only truly general and formal one, and to this we shall confine the name of *Definition.* We do not of course restrict ourselves to the scholastic limits above mentioned, but wherever a name admits of analysis in respect of its signification, there we shall claim a definition. But it must be understood at the same time that definition is but one

[1] Locke says (III. iv. § 6) that "a definition is nothing else but the showing the meaning of one word by several other not synonymous terms." This is a very wide account, but it must be admitted that he distinctly states that "names of simple ideas are undefinable."

way, and this a somewhat technical way, of conveying the correct meaning to any one who is in doubt about it. To raise the requisite experience at first hand, or by exercise of imagination, may often be the most effective plan. But where there is no other way than this available we shall consider that no true definition can be offered.

V. The question may now be raised, how and why Definitions should ever be needed: what, in fact, is the use of them? We found ourselves, the reader will remember, forced to make the preliminary assumption that language is understood in the same sense by all who use it within a circle of common intercourse. No other assumption was possible as a starting point for any science which dealt with the *communication* of thought. Now, when the Definition expressly disclaims doing anything more than expounding the meaning of a word, it would seem that it merely states what *ex hypothesi* every one who uses the word is already familiar with, and that therefore every Definition, from its nature, must be useless.

Some of the various answers to such an objection are obvious: we will only touch upon those which seem to involve questions of principle.

(1) For one thing, then, we have here a useful reminder of the advisability of regarding Logic as being an "applied" science. So long as we regard it as being purely formal the definition stands self-condemned as being obviously uncalled for and useless. The only ground on which it can be justified is the assumption that in its practical applications we shall frequently find that our fundamental postulate about language does not hold good. Many persons are constantly diverging, and all of us are occasionally diverging, from the common consensus of sound opinion about the meaning of words. Accordingly definitions are in practice very often of extreme value.

These considerations seem to afford an answer to a difficulty which has often been expressed. By admitting that the demand for a definition is a sort of *right* instead of a merely occasional concession to our mental indolence or frailty, logicians inevitably provoke a continued repetition of such a demand, and then the question arises, Where are we to stop?

How far back are we supposed to go in the assignment of such a series of successive definitions? The true answer is; You have no right to a Definition at all: the mere fact that you ask for one is in itself an admission of the general truth of our postulate about language: for on what other ground can you suppose that we shall know what you ask for by your question? That is, the definition involves confessedly, by virtue of its being asked for, an admission that you consider yourself sound on the subject of language generally, and merely want a partial and exceptional blank filled in. Accordingly all that the definition can propose to do is to supply a link connecting the missing or defective notion with the proximate notions which are presumed to be sound.

(2) But more than this. We are reminded of another practical departure, and one of a very important nature, from our fundamental postulate. Granted that each person knows exactly what he himself means, it is by no means true that we all speak the same language, even in the same country and at the same time. This opens up the question of technical or special definitions.

Special, or Technical Definitions. The Connotation of a term was accepted by us as being the 'meaning' generally assigned to it, allowing of course for the various reserves and explanations which such a way of speaking necessarily demanded. The right so to accept it rested upon the very reasonable postulate that language fulfilled fairly accurately its obvious functions as a medium of communication. But we must not forget that, side by side with the general language which a whole people have in common, there are a number of special languages in use by particular classes of the same people. Doubtless the great bulk of the words employed are common to both kinds of speech, but there are many which are peculiar to the special ones, and these peculiar elements fully deserve to be considered as a distinct language.

If it be asked what right we have to select a certain number of words found in (say) an English dictionary, and spoken by some particular class of persons in that country, and to talk of this selection as a distinct language, the reply is that a language *is* nothing but a set of words in use by some group of people, and that therefore in the case supposed

a distinct set of words, characteristic of or confined to a particular group of people, does certainly constitute a distinct language. Some of the words composing it are doubtless merely substitutes for those elsewhere in general use, but they are quite different words: others are peculiar to it, and symbolize notions unfamiliar or unknown to the bulk of speakers outside its range, in which case the symbols may either be familiar ones with a new signification attached to them, or entirely new variations or creations for the purpose. That is, an otherwise well-known sound may have a special sense put on it, thus becoming a new word; or both sound and sense may be new and special. Moreover, such a language may be the habitual speech of some sections of the community, or it may be the occasional medium resorted to for the discussion of special topics or for use on special occasions.

The reader will easily supply appropriate illustrations, so we need merely indicate, as instances of languages peculiar to certain classes, the slang of thieves, school-boys, and sporting persons, and the whole vocabulary of peculiar expressions required by sailors, miners, and indeed most classes of workmen. As instances of languages only required from time to time by those who want to speak of some very special subject, might be given the terms used by those discoursing of high mathematics or any other advanced science. Many people, doubtless, would be inclined to deny that these ought to be called special languages, and would describe them as English: on the ground that they are included in so-called English dictionaries, and are spoken by Englishmen. It seems philosophically more correct to say that such a dictionary includes other than English words, and that most Englishmen can and do speak other tongues than the English.

The bearing of these remarks on the subject of Definition is obvious. Just as the common speech universally spoken by the people of any country presupposes a commonly recognized meaning in every word: which meaning, when admitting of analysis, is called connotation, and the enunciation of this connotation is called Definition: so is it with each of these special languages. Their words have exactly the same characteristics and the same functions as any others. They yield

proper names, and connotative names, and these latter therefore admit of Definition in its true sense. Nay, as a matter of fact, it is probable that the work of defining these special words is easier, and the definitions are more accurate, than in the case of more generally familiar words. This might be expected on the ground that a word confined to a special class is much more likely to retain a uniform and fully recognized signification than one which has to do duty over a very wide area. The term *technical*, in its widest sense, I understand to apply to such words and such definitions as these: though, where the class of speakers is vulgar, or the subject trivial, we more often designate them by the word *slang*.

(3) The uses of Definition, as hitherto considered, refer only to linguistic conventions, and the divergences which actually exist amongst them. But when we look outside us, to the subject matter to which our language refers, we soon find that the practical aid which a Definition, and still more the process of framing a Definition, may afford, is enormous.

We have already pointed out that the central group of essential attributes,—namely those which constitute the connotation,—is surrounded by a much more numerous group, some of which are only just of less importance, or of less general recognition, than the few selected ones. The progress of knowledge has an obvious bearing upon this state of things, as it makes the tenure of the accepted attributes a somewhat precarious one. At any moment some discovery may be made which would certainly in time alter our relative estimate of these attributes, and therefore probably alter the conventional selection by which the meaning is determined.

This precarious character of even the best and most accurate current definitions has seemed to some writers so unscientific that they have endeavoured to remedy it by a rather strong proposal. They have been so convinced of the indeterminateness of the enquiry as to when an attribute can be said to have become 'universally accepted as a part of the meaning' that they have proposed to admit the attribute the moment *any person* has discovered it. Thus Mr Bain, one of the most philosophical supporters of this view, maintains[1] that "all newly

[1] "Deductive Logic," p. 70.

discovered properties are real predications on their first announcement, although immediately on their first communication they become verbal",—e.g. Faraday's discovery that oxygen is magnetic. This seems to me to be nothing short of a reduction to absurdity of the view in question. As a rule, a fact "immediately on its being communicated" is a very doubtful fact indeed, for only a portion of the statements taken from the last number of the appropriate scientific journal are finally accepted as true. But even if they do finally establish themselves, it is surely stretching the phrase beyond all license to call a proposition 'verbal' simply on the ground that it asserts a fact which most of us will never know, discovered by some authority of whom perhaps we never heard. In this sense we are perpetually hearing, and very possibly rejecting as incredible, assertions which are yet claimed as merely verbal.

We must regard the framing of a Definition as a rather serious matter. What we are doing is nothing short of amending, that is, changing the meaning of a name, and established names are not to be lightly meddled with. The process is like changing a law of a country, in that it does not merely concern an isolated act, but is instrumental in setting a custom. To decide the relative importance of the attributes demands a careful discrimination amongst their respective claims, and often presupposes the choice of some important leading principle in virtue of which they are to be judged. Any newly announced attribute therefore must not only be carefully tested in order to establish its truth, but we must remember that in incorporating it into the connotation we are setting a sort of official stamp upon its relative importance. In a word, Definition is the outcome of a great amount of research on the part of the framer, and is consequently a most important means of instruction on the part of the learner.

In the cases last considered the utility of the definitions, so far as their educative work is concerned, consists mainly in the judgment we have to form as to accepting or rejecting new qualities, as these from time to time come before us. There is a still more striking illustration however to be found of the way in which modifications of our definitions accompany the progress of knowledge. It is not necessary that there should be new facts discovered in order to lead to a revision of our

definitions. A new principle or theory will often effect a complete change in the order of dependence or importance in which the attributes are regarded.

Some of the most striking examples of the kind of transformation here alluded to are to be found in the province of Mathematics. Take the case of the Ellipse. If we had asked a Greek geometer to define it he would at once have replied that it is a species of conic section, and that the difference which distinguishes it from the others is that the cone is so cut that the section goes right across it. These attributes are permanently embodied in the name by which this class of curves was for long currently known, and from them all the other properties may be derived. But if we consult a treatise of sixty or eighty years ago (Hymers') we find that, though the old name is still kept up, that is, though the ellipse is still called 'a conic section', the curve is defined in a totally different way. The essential attribute now is that the curve is traced out by a point moving in a certain way, moving, that is, so that its distance from a fixed line bears always a constant ratio to its distance from a certain fixed point. And then follows the curious result that the fact that such a curve can be produced by cutting a cone by a plane comes out as the conclusion of a long mathematical deduction. The old essential attribute has now become a remote conclusion. We have to *prove* that a conic section is obtained by making a section of a cone. It is as if a demonstration were requisite to show that a quadruped has four legs. Again, a third starting-point might be chosen, and often is so at the present day. We may take as the essential characteristic of the class of curves in question that they are plane curves of the second order; that is, that their equations involve only the first two powers of the ordinates. If we do this, then both the above-mentioned characteristics become derivative instead of primary, and there is consequently another serious change in the essential properties, that is, in the definition of the thing in question. And all this change need not be in any way the consequence of the discovery of new properties: it may follow merely from a change of point of view.

Such a complete inversion as this of the order of precedence or derivation of the attributes is hardly possible outside the

range of mathematics, but a similar state of things exists in a less degree in many other directions. Any serious change in our philosophic point of view may, without actually adding to our knowledge of facts, yet bring about a very considerable redistribution as regards the relative importance of the attributes involved. For instance in the department of Zoology the rise of the doctrine of Development, as bearing on the mutability of species, has had a powerful influence in this respect. It is not so much that we have discovered new facts about the plants and animals as that a new theory has profoundly altered the relative importance of the facts that were known already. The biologist now lays an increased stress upon those characteristics which bear upon the past history of the organism, as illustrating the way in which each species is connected by actual affinity with others. This subject will however be best discussed, in another chapter, under the head of Classification.

(4) Outside the range of Mathematics and the Physical Sciences such modifications of Definition as are now under consideration are mostly the consequence of a very complicated process of change both without us and within us. It is not merely that we have discovered new facts, or formed a different theory about those previously discovered: there has probably also been a gradual change in the course of the events themselves. Take a single example by way of illustration, by tracing as far as we can the change that has, or may have, taken place in the connotation of the word *Pagan*.

Originally the word meant 'villager', and denoted the class of people who lived in villages. This was the essential attribute of the term, but along with this there were, as there always are, many unimportant and therefore accidental attributes: these villagers were less instructed, they lived less in contact with others, they probably ate different food, went to bed earlier, were stronger in frame, &c. Then a gradual change came on. Amongst these accidental attributes began to emerge a new one, that these villagers retained the old religion, whilst those in the towns mostly embraced a new one. Thus far the only change had been on what may be called the objective side, at least so far as regards those who mostly used the word; and, had this been all, there was no reason

why the new attribute should ever have quitted its place amongst the group of accidents. But now began what we may call the subjective or the mental change. The popular estimate with which the new attribute was regarded underwent an enormous change. The fact that any people worshipped the old gods became of infinitely more importance than the fact that they lived in villages. This attribute therefore soon came to be the prominent one when the word was used, and therefore took its place side by side with the old connotation, and finally superseded this. One more change then remains to be noticed, and this again occurred amongst the external phenomena. Up to this point the connotation only had changed: the denotation remained the same, but precariously so, as must always be the case under such circumstances. The people who lived in villages were, as a class, heathen; but inasmuch as the latter attribute had now sunk to the level of an accident (an accident, that is, of a villager) it might at any time come to be divorced from the others. As soon as the confines bounding the form of worship and the place of abode altogether ceased to coincide, the decisive change of connotation became marked in the unmistakeable way in which a change of application or denotation can hardly fail to exhibit itself, and the transfer of signification and application alike was then complete.

The above brief remarks will serve to show the very great gain that is to be secured by sound definitions, and still more by the process of investigating and drawing them up. We may say of them, to a somewhat less extent, what Hamilton has happily said of language generally in relation to our notions:—" A country may be overrun by an armed host, but it is only conquered by the establishment of fortresses. Words are the fortresses of thought. They enable us to realize our dominion over what we have already overrun in thought: to make every intellectual conquest the basis of operations for others still beyond." (*Log.* I. 138.) And, as a consequence of this, anything like finality in respect of our definitions is out of the question. Such a belief in finality naturally falls in with the philosophic attitude of the older logicians, and has found much encouragement in the treatment adopted by their Conceptualist successors. It almost seems indeed as if

these latter were anxious to show that the destruction of Realism in the sense of certain fixed archetypes of our ideas was no detriment to the *fixity* but only to the *externality* of the type. The denotation of our terms was always admittedly uncertain and fluctuating, but the connotation was supposed to stand out in contrast as comparatively fixed. The general impression conveyed is that of a world of ideas or notions which play the part of a mental currency: that pass from mind to mind by aid of words, and can be stored in the memory: that retain their value with little attrition or alteration, and may therefore at any time be subjected to analysis and resolved into their constituent elements by Definition.

Against this view the Inductive logician must take his stand. Fully admitting the desirability of leaving untouched the current words of the home and the market, he must maintain that as regards scientific words their growth is their life, and he must always be prepared therefore to reconsider his definitions in the light of either new facts or new theories.

These admissions have an obvious bearing also on the view, already noticed as entertained by some logicians, that what we should aim at in assigning our definitions is the enumeration of those attributes upon which the others depend. It is no doubt desirable to aim at this as far as we can, but we must remember that every estimate of this kind is liable to constant revision and alteration. As regards the dependence of one attribute upon another, we must remember that, in mathematics at any rate, we are apt to find that a number of attributes may be considered mutually deducible from each other, as was the case with the properties of the Ellipse. The decision therefore as to the selection of one or other of these must be determined on some other principle; say, in the light of some general mathematical theory. And then, again, outside the domain of mathematics it is often very difficult indeed to trace with certainty what *is* the dependence. In the case of the species of Natural Science, for instance, scarcely any such dependence can be found. There is plenty of correlation from which reasonable inferences can be drawn, but there is little in the way of necessary deduction. We do not now regard the properties of kinds as being arbitrarily connected by Nature; but, with the best of attainable information, such species offer a marked

contrast in this respect to anything which can be found in mathematics.

It appears therefore that this view, namely that primary attributes are to be selected, from which the subordinate ones may be deduced, falls in with what may be called the too objective treatment of Logic sometimes advocated. When knowledge is absolutely complete,—and when therefore Logic is entirely superseded so far as its utility is concerned,—we may be in a position to say in every case what is really primary and what is derivative. But till then there can be no final judgment as to the relative importance of such qualities as we may at any given time have discovered. They may be caused to change places in this respect, at any moment, by the breath of a philosophical or scientific theory passing over them.

VI. *Characteristics of a good Definition.* These may be inferred very readily from the nature of a definition, and there has been so little variation of opinion as to the main requisites that the traditional rules may be adopted almost without alteration. The rules for good definition are most conveniently given in the form of a series of precepts for the avoidance of certain faults, as follows.

(i) The Definition should not contain *more* than the connotation of the term in question, as otherwise we may unduly restrict[1] the class to which the name refers. We purposely use the words 'may', rather than 'must restrict', in accordance with the conclusions of p. 176. We are speaking of actual denotation, not potential, and must therefore bear in mind that attributes having the habit of occasionally coalescing in groups the enunciation of more of them than is necessary may sometimes happen not to reduce the actual denotation. It will be noticed that the speakers are here supposed to be in general agreement as to the denotation of the name, and that in framing our definition we want to select just the connotation which will permanently suit this denotation.

Paley's definition of virtue ("doing good in obedience to the will of God and for the sake of everlasting happiness") would, in the opinion of almost every ethical writer, be open

[1] 'Unduly', because it is taken for granted that we are in general agreement as to the application or denotation of the name.

to this fault, on the ground that the ultimate motive of the action formed no part of the meaning of the term and therefore unduly restricted its application.

(ii) So again the Definition must not contain *less* than the full connotation, or we are likely unduly to extend the class denoted by the name. This is perhaps the commonest fault of any, as we are apt to feel satisfied if our definition covers the cases we have in our immediate view, and to omit to examine whether it does not also admit something else which we were not at the moment thinking of.

(iii) Another fault consists in what is called "defining in a circle"; namely, introducing into the definition either the *definitum* itself, or some exact equivalent for it: in other words, offering a synonyme under the guise of a definition. It has been often pointed out that we find ourselves under a great temptation to fall into this error, owing to the existence in the English language of so many synonymes derived sometimes from the Teutonic and sometimes from the Italic contributing elements.

Johnson's definition of net-work, in his dictionary, is a well-known illustration of this fault;—"anything reticulated or decussated at equal intervals, with interstices at the intersections". Another amusing instance is offered by Pearson's definition of Belief at the commencement of his work on the Creed:— "Belief is an assent to that which is credible as credible".

In objecting to the substitution of synonymes in the place of true definitions, we must avoid pedantry. The above examples are fair enough game, for they are offered by men who should have known better: but many explanations may take the form of definition by oversight, being intended to do no more than substitute a familiar for an unfamiliar word. Any offer of a definition, remember, involves a certain inconsistency, unless the term be taken from some foreign or technical language. A call for a definition in any other case presupposes an ignorance of some word in common use. There are many ways of removing this ignorance practically: by definitions, descriptions, substitutions of synonymes, translation from other languages, exhibition of the objects named, and so forth. Of these the first, or logician's method, is a very

technical one, but it is probably far the best way of teaching and retaining the accurate usage of a word.

(iv) Another defective substitute for a definition is commonly recognized in the *Description.* As remarked just above, there need be no harm in descriptions, provided they do not profess to be more than they really are. Their function is to enable us simply and readily to recognize any object denoted by the name, or rather, as we ought to say, any object at present denoted by the name. For this purpose they may seize upon some very marked peculiarity which *at present* happens to distinguish the objects in question, but their validity is consequently very precarious outside the limits of their original application. Thus it would seem a serious error to propose, with De Morgan, as a definition of an elephant that it is an animal which naturally drinks by drawing water up its nose (if the trunk be, physiologically, a nose) and squirting it into its mouth. It may be a happily distinguishing mark at present, but no one would admit a new species to rank with the elephants on this ground alone.

(v) Finally, a fault often insisted on is that of using negative terms instead of positive in the definition. The objection to the former is partly connected with the old dislike to "infinite" terms; that is, it springs from the assumption that a negative term must necessarily be less definite than a positive one. That a positive name is, as a matter of fact, in most cases the narrower and more determinate, has been fully admitted already. This arises simply from the fact that we naturally want to name such narrow and determinate classes first, and of course choose positive names for them. What we should really be doing, therefore, if we proceeded to give negative names to these, would be to propose a *double* negative. And this, we may take it, is what the objection before us is really aimed at. If we want to define the miscellaneous class left by the subtraction of some other class, there is probably no better way of doing it than by a negation:—thus an 'alien' is, in England, described as one who is *not* a British citizen. But if we were to define a citizen as one who is not an alien, we should really be employing a double negation; since 'alien' though not negative in obvious form, involves, as just remarked, a negative conception, i.e. is reached by a process of negation or exclusion. All double

negation of this description is of course to be avoided on the ground of its awkwardness.

VII. *Essential and Accidental Propositions.* We must now take account of certain other propositions closely allied with Definitions; or rather, to speak more accurately, we must take account of that wider class of propositions out of which Definitions have been selected as a special kind.

The Definition, as we saw, is a proposition which declares the connotation,—the full connotation,—of a term. There is clearly therefore a wider class of propositions which predicate of a subject some portion, more or less, of its connotation; the definition being the extreme or limiting case amongst these. There are a number of synonymous expressions used by different writers for the distinction between these propositions and others:—verbal and real: essential and accidental: analytical and synthetical: explicative and ampliative; and others. These all mean substantially the same thing, indicating at most trifling differences in the point of view from which the propositions are regarded. Thus, for instance, when a proposition is called 'verbal' we mean that it gives no information except about the use of a *word*, and therefore gives no information at all except what was presupposed in the intelligent use of the word. When it is called 'essential' we mean that it deals only with the *essence* of the subject, in the sense of 'essence' previously explained. When it is called 'analytical' we regard it as starting with the subject as a datum and simply analyzing this; that is, the predicate only contains portions of the whole which constitutes the subject. And when it is called 'explicative' we mean that it explicates or unfolds the connotation of the subject. So, on the other hand, the contrasted propositions are called 'real', because they give, or may give, real information as opposed to verbal: they are called 'accidental' because the predicate is only an accident of the subject: they are called 'synthetical' because, instead of analyzing the contents of the notion or term, they attach to it predicates not presupposed by it: and they are called ampliative because by thus adding on to the subject they enlarge the subject notion.

Of these various synonymous expressions the antithesis between 'essential' and 'accidental' is perhaps the best. It is most in accordance with the traditional nomenclature of the

subject; and moreover,—when we have once recognized and allowed for the change of signification of the 'essence' of a term,—it points most directly to the main characteristic which distinguishes between the propositions in question. We shall therefore as a rule employ this pair of correlatives.

The statement of the above distinction is easy enough, the difficulty as usual lying in the application of it to certain doubtful cases. This difficulty is of course precisely similar to that which arises from the same cause in the application of our definitions, but the importance of the subject will amply justify the expenditure of the space required for a slight further explanation.

(1) Can this distinction between the essential and the accidental be applied to proper names? Proper names we have throughout maintained to be without connotation, and therefore on this ground the distinction would seem to be quite inapplicable. This is not the opinion however of all the authorities. Thus one logician (Dressler) lays it down that "judgments upon Alexander by his contemporaries were analytical, by us they are synthetical", and Mr Bain gives, as an instance of a truly verbal proposition, "Homer wrote the Iliad".

The former assertion is surely quite inadmissible. It apparently supposes that 'Alexander' was a sort of limited concept or notion, which could be completely realized in all its details by contemporaries, so that every attribute assigned to the subject was already commonly recognized as being there, whereas to posterity all these attributes, beyond a few which we may consider as essential, have faded away and been forgotten. This is a doctrine equally at variance with fact and with the usage of proper names. Bain's example is of a different kind, and belongs to the interesting class of[1] extreme or limiting cases which are often found so instructive, though logicians are far too prone to neglect their consideration altogether. It is probably contended here that 'Homer' is simply the name of the author of the Iliad whoever he may have been: that the name *means* nothing but the authorship of that poem. The plausibility of this contention rests on the following

[1] A number of such extreme cases will be found discussed in my *Symbolic Logic.*

grounds. Ordinary proper names denote some individual who is presumably known in many and various ways to those who name him. Accordingly they suggest from time to time many very distinct attributes to different speakers and hearers. The multiplicity of these attributes, their constant change, and the fact that there are always many in the background, to set off against the one predicated or in any other way suggested at the moment, are sufficient to guard us against assuming that the name can 'mean' any one of these rather than any other. But in proportion as the individual comes to be known or referred to under one dominant characteristic there is a gradual tendency for the name to suggest this attribute, and for any proposition asserting it to seem familiar and unnecessary. Now suppose the extreme case of some individual who is known to us by some one characteristic only. The otherwise unknown author of some ancient work stands in this peculiar position. He doubtless possessed as many attributes as any one else in his time, but inasmuch as he cannot now be thought of, or referred to, except through the one known attribute, the name naturally suggests this attribute with such persistency and force as almost to claim this as its meaning.

Such cases, like most extreme ones, offer a choice of alternatives. We may on the one hand continue to regard the name as simply denoting an individual, accepting the awkwardness of the fact that nothing but the one attribute can be assigned to him. On the other hand we may admit that the name 'means' this attribute. But in this case we ought still to retain the doctrine that proper names have no connotation, and to insist that the name had been taken out of the rank of proper names, and was placed in that of the significant or connotative.

(2) Can there be 'accidental predication' in the case of non-existent or imaginary objects? or, as Mansel puts it, can imaginary notions be the subjects of any but analytical judgments? The view of those who maintain that imaginary subjects can have none but essential predicates assigned to them is somewhat of this kind. From the very nature of such a subject it is supposed that we are obliged to stop short at the notion or concept; for there is no reality underlying it (whatever the test for such reality may be) and therefore no fresh appeal to experience can be made. The concept or notion

itself, thus taking the place of the reality, has to supply all the information obtainable about that subject. Accordingly the attributes possessed by the subject must be relatively few, and their number though thus limited must be regarded as complete, for the experience which should from time to time add to their number is here precluded. All the attributes are therefore concluded to stand upon the same footing; that is, each is regarded as essential. All this is quite true, but it nevertheless seems to me that such a view overlooks the true character of essential attributes. It does not follow that all the attributes must be essential, because they are all equally obtainable by simple appeal to the notion in the mind. Take, for instance, the example of a griffin. If I want to pourtray one I am bound to give it claws and wings, because these are implied in the name; but I may add on, according to pleasure, a multitude of such accidents as colour, attitude, size, and so forth. The distinction is as familiar to the heraldic painter as it is to the artist who designs for a work on Natural History. Just as imaginary notions admit of definition as accurately as real ones, so do they admit of accidental predication: it is not in these respects that the one can be distinguished from the other.

Mill indeed strongly maintains the opposite view. In discussing an example (about dragons, as being serpents breathing flame, Vol. I. p. 164) he says that we might try to state the case "on the hypothesis that the name serpent includes imaginary serpents. We shall find that it is now necessary to alter the predicates; for it cannot be asserted that an imaginary creature breathes flame: in predicating of it such a fact, we assert by the most positive implication that it is real and not imaginary". This is a repetition of the doctrine we have already discussed in an earlier chapter. It seems to me that Mill lays it down in far too uncompromising a manner that we are concerned with only *one* test of truth or reality, namely that of sensible experience. That this is the one paramount consideration in physical science is of course indisputable, and there would be some consistency in adhering to such a view in a work dealing solely with Inductive Logic. But Logic generally should be more catholic in its toleration, and should be prepared to accept as 'real', for its purposes, anything which is guaranteed by some

kind of test or standard, without insisting that this test should be the physical one. Speaking as physicists we should avoid mentioning dragons at all, except with a view to accounting for the origin of the belief in them. But if, as logicians, we had gone so far as to name them, we should not feel that we were making a damaging admission in their favour by ascribing the breathing of flame to them.

(3) *Verbal Disputes.* Another aspect of the distinction between essential and accidental propositions is set before us in the existence of what are commonly called 'verbal' disputes. As to the actual occurrence of disputes of this nature very opposite opinions have been expressed; for whilst some (with Locke) maintain 'that the greatest part of the disputes in the world are merely verbal', there are others who, with more love for subtle distinction, go to the opposite extreme and maintain (with De Quincey) that they have never in the whole course of their lives met with such a thing as a merely verbal dispute.

By a verbal dispute, as I apprehend it, is meant one in which there is no difference as to matters of fact between the disputants, and wherein therefore, if they dispute at all, there must be a difference between them as to the exact meaning of the words they are using. I should much doubt the frequency of such disputes as these. That there are cases in which people notoriously use words in different senses is of course obvious,—if, for instance, an Anglican and a Romanist were to dispute as to whether such and such a rite was enjoined by "the Church" they would not be likely to go far in concert. But even here it is perceptible, what would be still more obvious in most instances, that a different usage of words almost necessarily entails different convictions as to facts. Differences of conviction as to something much deeper than words separate the Anglican and Romanist, and these differences are at work in making them use the words in different senses. The interaction between the notion or word and the objects denoted by the word,—the way, that is, in which we modify our notions by the acquisition of new facts, and acquire our knowledge of new facts by having our notions cleared and defined,—is very close and constant. Hence it becomes very difficult to find two persons both competently acquainted with the facts in question, speaking the same language, and yet definitely assigning different connota-

tions to any of the common terms they use. To take a well-worn example, mentioned by Locke: Is a bat a bird? It is surely almost incredible that there should be a dispute carried on between two persons, both of whom are acquainted with the leading facts of the bat's physiology and who merely use the word 'bird' in different senses: one, for instance, holding that this word implies no more than a power of flying, and the other that it implies certain physiological or morphological characteristics. What we should of course expect to find in such a case is, not so much a difference of meaning attached to the word 'bird', as a difference of knowledge about the nature of a bat, the one knowing, and the other being ignorant of, the affinity of a bat to a mouse.

One of the very few instances we can find in which a dispute might rage about nothing beyond the meaning of a word, is raised by the old tale, which repeats itself under so many varying forms, as to the *sameness* of an object. The knife,—or ship, or stocking, or whatever else it may be,—of which all the constituent parts have been successively replaced: is this the 'same' object finally, or is it not? In such a case as this we are avowedly in agreement about every relevant fact, so if any dispute can be achieved at all it must turn entirely upon different significations of the word 'same'.

(4) *Contradiction in terms.* As this old logical expression still finds a frequent place in common language it deserves a few words of notice here. It is only another side of the same facts referred to throughout the above discussion. Just as an essential or verbal proposition is one which predicates of a subject a term already involved in its connotation, so the contradiction in terms,—or *contradictio in adjecto*, as it generally used to be called,—is one which predicates of a subject any attribute which is, or can be shown to be, contradicted by the connotation.

Of course blunders of this description are not likely to be made except through lapse of attention or misapprehension of some kind, since they flagrantly violate the fundamental assumption as to general agreement about the meaning of words amongst those who are in communication with each other. The misapprehension probably arises as often as not from mere pedantry; from the habit, that is, of persons persisting in

accepting a term in its etymological signification when they must know that the current signification has drifted far from the source, or adhering in general discourse to a signification which is only admitted in some technical circles. Thus a writer in a high-class journal once gravely asserted that "free institutions are a contradiction in terms": what the writer meant being that institutions are founded upon laws, and laws involve restraint,—assuming therefore that 'free' in the above sentence meant "without any restraint". So an old writer tells us that "a perfect creature is a contradiction in terms", meaning presumably that the fact of having been created is necessarily a diminution from absolute power, and that this is in itself a diminution from perfection[1].

[1] Some instruction perhaps may be gained from a notice which I once saw, contained in a printed list of changes at a Railway Station, to the effect that "On Sundays the 10 A.M. train will start at 9.30". To begin, what is it that constitutes the unity of a train, or makes it the 'same' train day after day? Clearly not the physical identity of engine or carriages; nor the personal identity of the driver, stoker, and guards; still less that of the passengers who travel by it. Not one of these need be the same one day and another. Nor have we here the connecting link of *continuity*, which is the main ground of identification in most cases where the materials are entirely changed, as in organized bodies, and in the ship, or the stocking of the common example. In all these cases each new element is built up into the fabric, and, so to say, makes itself at home there for some time before its turn to depart draws nigh; whereas the train of to-day may for aught we know have been turned out fresh from the workshop. The real unifying element here is of course the *time* element, that is, the relative situation in the period of time which we call the day. And this itself is a somewhat artificial conception, depending on rather accurate measurement of time. No savage could thus identify the train as he could the stoker. The reason for our thus making the agreement in time the ground of identity is obviously that, in modern life and travelling conditions by rail, the time of starting is the one important differentiating circumstance. No one, for instance, would mean, by saying that he went to New York every year by the 'same steamboat', what he would mean by saying that he went every day to London by the 'same train'. The difference between one boat and another is very important, and there is not as yet any opening to portioning out the day by the departure of vessels at sea as we can by the departure of trains on land.

Considering that the time element is the only determining and individualizing one in the train service, the Sunday notice is a remarkably neat instance of the logical 'contradiction in terms'; but of course what it really implies is that we must not be too precise in our determination. A train does not sacrifice its identity by moderate unpunctuality, and a margin of half-an-hour or so may perhaps be fairly allowed before we begin to dispute whether it really *is* the same train or not.

(5) It is so important to be able to distinguish between what is verbal and what is not, in these matters; and also (what the beginner may be liable to overlook) to realize to how great an extent the verbal character of a statement may be dependent upon conditions of time or space, that we will spend a short time in the detailed discussion of what seems a really instructive example. A good illustration, then, of the unsuitability of this conception of the essence being that without which a thing would 'cease to be', instead of its being merely that without which we cease to apply the name, is found in the names we give to the different physical states of one and the same substance. We will take it for granted that most, if not all, substances can exist in any one of the three distinct states known as solid, liquid, and gaseous; the passage from one of these states to another being determined by conditions of temperature and pressure. A change from one of these states to another is supposed to make no difference in the molecular elements of the body, but merely to alter the mutual behaviour to each other of the molecules: that is, each molecule, whether taken (say) from steam, water, or ice, will be exactly similar, whereas when the water is analyzed into oxygen and hydrogen its molecules are broken up.

This being so, how should one expect that a scientific language would name these states? Naturally we should look for one common name for the substance, whilst its different conditions of existence were indicated by adjectival modifications or additions. It might be called respectively solid, liquid, and gaseous water. Of course no such usage prevails in this case,—at least in any language spoken by the inhabitants of temperate climates,—but we give radically distinct names to the three different states. Hence we are liable to be led into such statements as that ice would cease to be ice if it were not rigid, and so forth.

Now compare with the above the case of quicksilver. Like water it exists in the three states, solid, liquid, and gaseous; but as two of these are very unfamiliar to us they have acquired no distinctive popular names. We employ one name all through, applying modifying adjectives to it, and accordingly speak of the quicksilver as being frozen or vaporized. Hence it results that whereas the physical condition, as regards

temperature, is taken as a part of the essence of water, it is not so of quicksilver. If any one had contracted to supply us with water we should presumably have a right of action against him if he brought ice instead: certainly we should in the midst of a severe winter; but if we had contracted for a given weight of quicksilver we could not reasonably refuse it if it were brought to us frozen. Here, remark, the familiar condition to us is the liquid one of quicksilver. We might equally have chosen the case of iron where the familiar state which gives the common name is the solid one, so that we distinguish the other states by describing the iron as melted or vaporized. Or we might have chosen carbonic acid gas, where the gaseous state is the only one at all familiarly known.

In these cases the bare name by itself doubtless *suggests* the customary condition of the body, since this is the only one with which we are familiar; but it certainly does not *mean* it, and accordingly there is no contradiction involved in speaking of frozen and vaporized quicksilver, or melted or vaporized iron. Nor, for the same reason would it be actually erroneous, though certainly highly misleading, to speak of frozen iron, or melted quicksilver, when we wished to refer to the ordinary condition of these metals.

What we find, then, is this. Where the three states have been familiar to man from time immemorial, and have presented themselves to us in innumerable important relations, we find an entirely distinct name assigned to each. In this case the physical condition is of course recognized as a part of the connotation or essence. Where one only of the states is familiar to us this one commonly appropriates the name, and the others are assigned by aid of the requisite adjectives. *Which* state thus appropriates the name depends of course upon circumstances, which in this case are determined by temperature. We naturally talk of 'frozen' quicksilver and of 'melted' iron; but did we usually live at a temperature much below zero we should speak of quicksilver, in the condition in which we now commonly see it, as 'melted'; and if we lived in a temperature above 3000° we should have to describe our now ordinary iron as 'frozen'.

Another good illustration of an analogous kind is to be found in the words indicative of the times and seasons in

different parts of the world. 'Day' and 'Night' distinctly imply the light and dark portions of the twenty-four hours: this is of their essence, and therefore there can be no hesitation as to their application in North and South latitudes, and in opposite longitudes. No awkwardness is felt in speaking of 'a polar night of six months'. Similarly with Midsummer and Midwinter, which accordingly take place simultaneously in England and Australia respectively. But Christmas and Lady Day, on the other hand, are mere proper names, denoting certain dates in the year, and having no further signification: *they* accordingly retain their times unchanged, so that Christmas, in Australia, comes in Midsummer.

What we have to remember in these and all similar cases is that language is devised to meet practical necessities; that consequently, wherever any state or condition of things is sufficiently clearly marked off from others and is sufficiently important to us, it will infallibly acquire a name of its own, either an entirely distinct name or a modification of some other name. The name will then come to imply the customary state or condition; in other words, this will be of its essence. But the state or condition thus implied will almost invariably exist under relations of time, place, or circumstance. When these vary or are abandoned, the name of course no longer applies. And if, as may very well happen, the relations which thus determine the familiar state of things are very widespread or durable (as in some of the above instances) there is always a certain shock to the mind in the application of the name under altered circumstances.

VIII. A few words may be inserted here concerning the best mode of enumerating the various attributes which constitute the Connotation or Essence of the name to be defined. In speaking of this enumeration we must remember that an attribute is not an irresolvable entity. It is an abstraction indicated and retained by a name. We may find it convenient to use a single name for it, and thus regard it as a unity, but this unity resembles that of some fibrous substance which we can split up again and again. The lines of such fissure depend upon our own choice, regulated by the resources of the language at our command. The definiteness of language, that is, the fact that it necessarily consists of a determinate number

of discrete words in every sentence, gives an air of completeness to the process of analysis at every stage, but there is very little which corresponds to this completeness in nature, or indeed in our own mental processes except in so far as they are determined by the use of language.

(1) The consideration just mentioned is important in its bearing upon the old scholastic account of Definition. The definition of any term was assigned, as the phrase was, "by genus and differentia". That is, recognizing that any assignment of attributes for the purpose could not be regarded as ultimate, but would admit of continuous further analysis, they cut matters short by going back only one stage. What is done on this plan is to regard the class whose name is to be defined as a species, select its next highest genus, assign the connotation of this as far as possible by a single term, and then merely add on to this the 'difference' which distinguishes the species from its genus. Thus 'man' contained in ultimate analysis many attributes, but inasmuch as 'animal' was the next higher class, or 'genus proximum', the bulk of them were contained in this term. The remainder were assigned, with equal brevity, by the differentia 'rational'. Hence 'rational animal' was the complete definition.

There is more than convenience or simplicity in this plan. It is really in many cases the most rational and scientific. Remember, as already explained (p. 280), that the offer of a definition at all represents a departure from the full rigidity of the assumptions with which we are forced to start in Logic. If we are not to suppose that people know the meaning of the terms they use, we will keep as near to this supposition as we can by assuming that they know the meaning of every term except the one in question, and there is then all the requisite propriety and completeness in the offer of the genus and differentia by way of definition.

It should be remarked here that this process did not seem so indefinite, in one respect, to the old logicians as it may to us. When we hear of a proximate genus, or next higher class, we may not know at once where to look for it. But with them Logic partook of the accuracy of a professional subject the students of which were in general accord as to the subdivisions it gave rise to. They would therefore have but little difficulty

in selecting from the scale of classification afforded by the Predicaments, *the* proximate genus, and as little in assigning the corresponding differentia.

The above is the complete and formal mode of assigning a Definition. When however we merely want practically to distinguish and recognize the class in question, there are other modes available: one of these being an imperfect definition, and the other not, in strictness, a definition at all.

The former of these, which may be called an essential but incomplete definition, enumerates a portion only of the connotation. That is, it falls short of the complete definition by assigning a portion only of the full differentia, or if it assigns this in full it fails by tacking it on to some genus higher up in the scale than the proximate one. It must be remarked that the satisfactoriness or otherwise of such a plan must depend upon material conditions. Every attribute, short of the full complement, with which we may permit ourselves to be satisfied represents a potential difference of denotation, and therefore possible failure of the definition:—whether such difference actually prevails must be decided by experience. The consideration here before us is in fact precisely the same as that already noted when we were discussing Connotation and Denotation in their mutual relation. It was shown that owing to the fact of attributes going, so to say, in bundles together, it would often happen that the omission to record all the group would not entail any practical loss: the remainder would be found to present themselves without being specially called for, provided that one had been already secured. Hence, in so far as the practical distinction of the class in question from all other existent classes is concerned, it is often enough to assign a few only of the essential attributes.

The mode of Definition by Genus and Difference, just mentioned, is one of time-honoured antiquity, and seems also one of the most natural and obvious in many cases. Given a general knowledge of the subject in hand and of the language in common use, but given also one of those temporary lapses which make the science of Logic of practical utility, and this method seems the one which would most readily be adopted. What more natural than to "try back" a stage, in case of such occasional ignorance and uncertainty, and say, Well you

know this,—appealing to some simpler and more general class, —start with it and modify or determine it in such and such a way, and you have what you are seeking after?

There are however several other ways of assigning Definitions, or what practically come to the same thing as definitions, in the way of making us understand the usage and meaning of words.

(2) *Description.* One of these remaining modes comprises what are commonly called *Descriptions,* in contradistinction to Definitions proper. We resort to them when we determine the class, not as above, by an insufficient enumeration of essential attributes, but by the assignment of attributes which are no part of the essence: in other words, by inseparable accidents. The possibility of resorting to either of these two methods rests upon one and the same fact, mentioned above, viz. that the distinguishing attributes which are found to separate off class from class in nature very frequently present themselves in groups. We may purposely mention only one attribute, but we shall probably find that along with it several others are unavoidably taken into the bargain. Hence it follows that two very distinct assignments of attributes may practically result in one and the same determination of a class. Were we concerned with the potential only, that is, did we propose to place all conceivable classes of things upon the same logical footing of reality, this would be otherwise. The Description would then at once become a Definition, but it would of course be the definition of a very different class-term; for a *potential* Denotation necessarily varies with every slightest variation of Connotation. As things are, however, a group of attributes suitably chosen, or even a single attribute, will often serve to assign the limits of a class as accurately and much more concisely than a full formal Definition. Of course there is always the drawback, in these cases, of the applicability of our Description being precarious. It may serve well enough to delimit the desired class here and now, but a change of circumstances may at any time cause the group of attributes, upon the coexistence of which the validity of the Description depended, to fall asunder, and then the Description instantly fails to apply. For instance, it is no part of the meaning or essence of 'midday' that the sun should then be due south, but

in our hemisphere this is an inseparable accident, and such a circumstance will therefore serve to determine that time of day. But move into the southern temperate region and the sun will be due north at midday, and the Description fails to answer its purpose.

The practical test in fact, when we wish to know whether any proposed Definition is a true one or not, is to try whether by conceivable variation of circumstances we can cause it to break down, by its exclusion of what we are resolved to retain or its inclusion of what we are resolved to reject. Thus, to recur to a very venerable old logical joke, when it was proposed to define 'man' as 'a featherless biped', a plucked cock was exhibited by way of confutation.

It is worth remarking, in passing, that etymologically Descriptions would have a fair right to rank with true Definitions, inasmuch as all that is thus implied by this latter word is the assignment of the limits or boundaries of a class. Accordingly, if we listened to such an etymological claim,—which of course we must not do in Logic,—*any* indication whereby those limits were assigned would deserve the same name. In point of fact the allied words 'definite' and 'definitely' have both retained more of the original signification than has survived in 'Definition'.

(3) Definition by *Type*. There is a peculiar form of definition, or what is often ranked as such, in which the group is associated around some selected individual chosen as a Type. This form is almost necessarily confined to the natural classes furnished in the Biological Sciences. Attention was first called to this way of grouping,—so far as the logical aspect of the question is concerned,—by Whewell; though it must be noticed that he expressly denies to it the title of Definition[1]. His words are as follows:—"A Type is an example of any class, for instance, a species of a genus, which is considered as eminently possessing the characters of the class. All the species which have a greater affinity with this Type-species than with others, form the genus, and are ranged about it, deviating from it in various directions and different degrees. Thus a genus may consist of several species, which approach very near the type, and of which the claim to a place with it is obvious; while

[1] *History of Scientific Ideas*, II. 121.

there may be other species which straggle further from this central knot, and which yet are clearly more connected with it than with any other. And even if there should be some species of which the place is dubious, and which appear to be equally bound by two generic types, it is easily seen that this would not destroy the reality of the generic groups, any more than the scattered trees of the intervening plain prevent our speaking intelligibly of the distinct forests of two separate hills."

The main thing to notice about this is the wide departure which it involves from all other kinds of definition or description, in that, instead of trusting to the resources of language and endeavouring to supply the deficiency in respect of a single word or notion by an appeal to adjacent words or notions, it abandons this course altogether and appeals to some kind of sensible intuition. It says practically, 'I will show you what I mean by the name in question: I mean something like *that*', pointing to an object before us. The process is really the same as that by which the child acquires knowledge, and by which we are all apt to acquire it about things very unfamiliar to us, or so simple in their nature that it would be difficult to refer them to any higher genus. It is not for a moment suggested that such a resource is not most serviceable in many cases,—in some cases it may be the only resource available,—but we must not forget that from the logical point of view it is less of a true Definition than is the so-called Description[1].

[1] What are the actual agencies which keep these allied classes from continually interpenetrating till they almost, so to say, dissolve into each other, is a profoundly interesting question. Much enquiry has been devoted in this direction as regards the species of Natural History, for instance by Darwin in his researches on Cross-Fertilization, and the whole subject is being now carefully studied. In the somewhat analogous case furnished by groups of human actions we can see our way more easily. Law and Custom often tend actually, and even designedly, to curtail the never-ending modification which the complicated and shifting circumstances of life would otherwise bring about. If, for instance, the letting of houses were left absolutely to the caprice of the innumerable owners and tenants, we can realize how impossible it would be to make any permanent subdivision or classification of leases, or consequently to define them. But the control of the law, and the adherence to custom of professional agents, have a decided tendency to correct the vagueness that would otherwise spring up. This helps to keep the different classes permanently apart, with a distinctness somewhat resembling that found between the various 'species' in Nature.

(4) The last kind of Definition deserving notice is the Genetic or Constructive one. This is really a successful instance of doing what was already alluded to as only occasionally practicable, namely, enumerating the primary attributes upon which the others depend; but it is a very complete mode of doing so. Instead however of simply mentioning the one or more primary attributes from which the others may be deduced, we go to the root of the matter by describing a process by which these primary attributes may be secured. This is a plan which cannot often be successfully carried out except in the region of mathematics, but there it is often resorted to, and indeed whole treatises have been written from this point of view.

One or two simple examples will serve to display the method in question. Euclid's definition of a circle, for instance, is that it is a plane curve every point of which is at the same distance from a certain fixed point, viz. the centre. This says nothing expressly about how to draw such a figure, but it instantly suggests a method of doing so :—Take two points at a constant distance from each other: keep one of them fixed whilst the other revolves around it, as in the common pair of compasses, and the thing is done. In the case of the most usual practical method of tracing out an ellipse, the mode of production is a little more remote from the definitions commonly given. Of course we might, if we pleased, start with the definition that it is a plane figure every point of which has the arithmetical sum of its distances from two other fixed points constant, but this is not a very convenient starting-point for geometrical purposes. For constructive purposes, however, it is one of the simplest possible, and every skilled gardener who was told to mark out an oval bed in a grass-plot would naturally adopt it.

Where definitions of this kind are most naturally resorted to is in solid geometry, as we may thus often avoid a long verbal description. Even Euclid, it may be remembered, has adopted this mode here, departing in this respect from his practice in plain geometry. The saving of trouble in the case of many figures is immense. Thus it would be a tiresome process to give a purely verbal account of the distinctive characteristics of even such a simple figure as that of a common round

ring, like an anchor or wedding ring. But it is extremely simple to say, Let a circle revolve round an axis in its own plane but outside it. And when we come to more complicated figures the saving is greater still.

In the majority of available cases the genetic or constructive definition is really a natural and simple one. It is either the most obvious mode of carrying out what every one realizes as the actual meaning of the figure, as in the circle, or it is only one step removed from this, as in the ring; but sometimes this is very far indeed from being the case.

Occasionally, for instance, in geometrical speculations, it may be convenient to offer a construction which is excessively remote from anything we naturally conceive of the figure in question. No more extreme instance can be suggested than the recent link-work construction for a straight line. For centuries it had been attempted to devise some geometrical method of converting a circular motion at one point of a system into an accurately rectilinear one at another point. The problem was only solved a few years ago by Peaucellier. No doubt the title of a little volume on link-work motion by Mr Kempe, "How to draw a straight line", is somewhat far fetched, since it is nearly certain that straight lines never will be so drawn, yet for theoretic purposes there is nothing to find objection to in the title. It might well be that in a regular geometrical treatise the most systematic way of fitting in the straight line along with various classes of curves, if we were expressly discussing link-work motion, might be by some such definition[1].

Sometimes this state of things is reversed, and we have an extremely simple constructive process for a figure which has given the geometer much trouble. The simplest instance perhaps of this is the cycloid. The path traced out by a point in the circumference of a carriage-wheel must have been suggested to the eye from time immemorial, and portions of such a figure may be seen traced out on the brick or plaster walls of

[1] Another very simple genetic definition of a straight line is given by saying that it is the path traced out by a point on the circumference of a circle which rolls inside another circle of twice its diameter. Circumstances are conceivable under which this might actually be the most convenient mode of tracing a straight line.

almost every narrow street by the scraping of the wheels. Nothing is simpler therefore than to give a constructive definition, but as the geometer knows the curve is not an easy one to describe otherwise, and it was late in the history of mathematics before its properties were at all fully understood.

CHAPTER XII.

DIVISION.

In the old treatment of Logic the subject of Division is generally found to follow closely upon that of Definition. Widely as the conception of these two operations, as they are required for Inductive purposes, has departed from that which tradition has handed down,—and the departure in the case of Division is far wider than in that of Definition,—it will be advisable still to keep to the usual order of discussion. We will begin with a brief comparison between these operations as regarded from the narrow and formal point of view.

I. Reverting then to the familiar fact that terms have, as a general rule, both Connotation and Denotation, we see that we may propose to analyze them under either aspect; but when we proceed to do this we see that these processes of analysis stand on a very different footing.

(1) Definition, for instance, is from the nature of the case both a direct and a formal process. It is direct, because the Connotation consists of, i.e. actually *is*, the attributes which we are therein proposing to enumerate, and which we must therefore presume to be actually present to the mind of every one who is fully informed of the meaning of the word in question. In point of fact, definition, as we saw, is open to the superficial objection that it tells us nothing, in telling us only what we are already supposed to know. It is, for the same reasons, formal. That is, a true Definition should not step outside the known connotation, nor therefore stand in need of any appeal to fresh experience; for such exceptional kinds of definition as those of a genetic character, and those which are commonly

called Descriptions, involve a departure from strict propriety of treatment.

That this purity of formal treatment cannot completely be attained is true. For instance, we noticed that one practical way of abbreviating the full process of enumerating all the items of the connotation, consisted in selecting some higher class or genus which included all but one of the constituent attributes, and then adding on this final one separately. This was the time-honoured method of assigning a Definition 'by genus and differentia'. Now it is clear that, if we attended to formal considerations only, there would be a serious ambiguity or indeterminateness in this process; for why should we select any one attribute rather than another to act as 'difference'? We should of course expect to have as many such varieties of definition before us as there were attributes in the Connotation. This, however, was never allowed in the old treatment. Practical considerations, partly arising from conventions of language, partly suggested by convenience and common sense, hindered any such excess of formality. It was always understood that there was *a* genus, to which each species naturally belonged, instead of a plurality to any of which it might equally be referred. Thus 'man' would never be referred to any other genus than 'animal', from which it was differenced by 'rationality'.

(2) Division, on the other hand, is indirect and material. It is indirect, because in strict formal logic it is the connotation which is regarded as primary, the denotation being dependent upon this and therefore secondary. And this could hardly have been otherwise. In a mental stage which might be characterized as one of great clearness and consistency of thinking, but at the same time as one which was much lacking in material information, it was only natural that peculiar stress should be laid upon that element which could best be dealt with by mere introspection. The connotation demanded no knowledge over what range the things which possessed it might be found, nor even whether there were any such things. Thus the mere meaning of the term Dragon might admit of decision without much trouble; the real task was only commenced when we undertook to say when and where they were to be found. Hence in great part, as I

cannot but think, the preponderating importance attached to the subjective side of the term, viz. the connotation, which is so distinctive of the old logic.

We have greatly altered our comparative estimate in this respect, and must regard the two aspects of the term,—the denotative and the connotative,—as being of equal importance, and as being both capable, according to the circumstances of the case, of taking the lead. If however we are asked which of the two must in general be regarded as the prior one, there can be no doubt that we must decide, with the old logicians, in favour of the connotation. We could not rationally start with a denotation pure and simple, and then try to fit on a connotation to it. The attempt to do this would in strictness involve our starting with a perfectly chance lot of things, and ascertaining what group of attributes they happened to possess in common. What we always really do, of course, even in our nearest approaches to a random choice, is to start with some principle of selection and to choose under certain guiding restrictions, and this clearly implies a certain priority of the connotation over the denotation. This holds true of every class which we compose or select, and therefore the process of Division is generally an indirect one as compared with Definition; it presupposes, if not a complete definition, at any rate some knowledge of the attributes possessed by the things which, through our process of selection, are to constitute a division of the assigned class.

Division, again, is material rather than formal; that is, it demands a fresh appeal to the subject-matter. This is obvious, for every attribute which is included in the connotation must be present in each of the individuals denoted, so that we cannot introduce any ground of separation amongst these individuals by the retention or omission of any one of these attributes. If we are to distinguish amongst them it must be by appeal to attributes which are accidental, that is, to such as are discovered by a fresh appeal to experience. It is this circumstance which has induced some purists in Formal Logic,—for instance Mansel,—to object altogether to the introduction of the process of Division.

II. For such reasons as the above it is obvious that very little can be made of this subject in its ungeneralized or non-

inductive treatment. Indeed what little has been said about it has always involved some transgression of strict formal considerations. Some notice however must here be taken of the old method of treatment, if only for the purpose of showing how and where enlargement is called for when we turn to a broader view of the subject.

(1) *The Porphyrian Tree.* The process of division found its main expression in what is commonly called the Porphyrian Tree. To understand the nature of this we must step back a moment to the doctrine of the Categories or Predicaments. It is often supposed that what was aimed at in these was the same sort of thing as is aimed at by the modern logician when he attempts a summary classification of all the objects with which his science proposes to deal. This however was by no means the case. The famous ten categories, whatever may have been the intention of their propounder, were by the later Aristotelians expressly declared not to be all-embracive. They only professed to be general heads of predication, that is, to be a classification of all possible *predicates*. Now, from the nature of the case, there may be things fitted to be the subjects of propositions which could not well be predicates, and these were omitted from the Categories:—'God', for instance, was excluded from the list. What was aimed at in fact was something very practical, the want of which must often have been felt in the disputations of the schools. When a given thesis was proposed for attack and defence the disputant was constantly in want of middle terms whereby to connect his conclusion with some admitted premise. The Categories furnished a convenient hunting ground for this purpose; and rules were given for guiding the disputant in his search, directing him to which class to appeal and how to limit his enquiries within its field. But it was never contemplated that every logical entity was to find a place in those Categories.

Another practical departure from the modern conception is to be found in the fact that no attempt was made to embrace the ten categories under one general head, that of 'being'. Each of the highest divisions was considered as truly '*sui generis*', that is, as belonging to the highest class of which account need be taken in Logic; which was reasonable enough from the practical point of view. But,

starting with any one of these upper classes, the division downwards was carried on with some care. As several of the technical terms still in use in Logic, and even in common life, are not easily intelligible without reference to the processes adopted by the older logicians we shall find it convenient to take a simple example. Start, for instance, with the first of the categoric classes, viz. that of *substance*. It is thus divided by Seton[1].

Substantia
corporea incorporea
Corpus
compositum simplex
Compositum
vivens inanime
vivens
sensile insensile
animal
rationale irrationale
Homo
Plato: Maro: Socrates.

Such an arrangement as this is called the Porphyrian Tree; a few remarks may be offered in explanation of some of its characteristics.

(i) For one thing, it may be remarked in passing, the *whole* of this arrangement was collectively called the Category. In the modern usage this name is confined to the uppermost class, i.e. 'substantia'.

(ii) It will be observed that though we make a dichotomy at every step, yet one of the two subdivisions is at once laid aside and subjected to no further analysis. This reminds us how distinct is the process thus indicated from that of modern Classification. What is desired in the latter is a complete enumeration of all the subsidiary classes. We start with no preconceived intention of reaching one such special or ultimate

[1] *Dialectica*: a small volume much used at Cambridge in the seventeenth century. In the *Aristippus* (about 1630) of Thomas Randolf, fellow of Trinity, a sort of students' chorus occurs in which are the words

"I'll be no more beaten for greasy Jack Seaton
Or conning of Sandersonus."

class rather than another. But here the rejection at every step of one of the divisions, and the retention of the other, shows a design of guiding our path towards a definite point: in the case in question what we want to reach is *man*. What we have before us, in fact, is a method of subdivision rather than a system of classification.

(iii) The process gave rise to certain technical terms. The line along which we thus direct our course in order to come down to the particular class or individual at which we aim, is called the *predicamental line*. The principle of selection adopted at each step in order to break up the class into two divisions is called the *fundamentum divisionis*.

(iv) The non-formal or material character of the process is obvious. Thus *compositum* does not contain, or in any direct way suggest, the attribute *vivens*. This latter is therefore an accident which has to be supplied by the special knowledge of the fact that some *compositum* is, and some is not, *vivens*. This attribute in fact must be a separable accident of the subject class.

(2) In the common, or Porphyrian Tree, we do as a matter of fact make our division into two contradictory classes at every stage, because this is the most convenient plan. But several of the old writers, e.g. Sanderson, point out that we need not be over particular in adhering to such careful dichotomy. Those whom they had in view in this protest were doubtless the followers of Ramus, whose innovations however in this respect, as in others, seem to be extremely small compared with the claims made by their leader and the outcry raised by their opponents. So far as any definite doctrine on this subject of Division is to be found amongst the Ramists, it seems to consist in little more than a recommendation to keep to a stringent dichotomy by always dividing into pairs of contradictories. This is certainly what is intended by those[1] who have, with Jeremy Bentham, spoken of "the matchless beauty of the Ramean Tree".

What these writers seem to overlook is the important distinction between dichotomy as a process for reaching some desired class, and a dichotomous final arrangement of our classes. As regards the latter no good reason can be offered

[1] Jevons' *Pr. of Sc.* 704.

why the subdivision should be thus bifurcate, and it would be the merest pedantry to insist upon making it so. In the arrangement of plants, for instance, most systematists start as the primary step with a three-fold division, namely that into exogens, endogens, and akrogens. Each of these three great families is then subdivided into a varying number of orders, these again into genera, and so on. In looking through such a scheme we shall find few, if any, traces of dichotomy.

Such a principle as this may sometimes subserve a useful purpose in testing a scheme of classification. But its main use is found in supplying a clue for the gradual detection of some class of which we are in search. This is a topic which will come before us more fully in the next chapter, so a very few words will suffice here. Consider, for example, the Analytical Key prefixed to such a work as Bentham's *British Flora*. We appeal to this when we have some plant actually in hand whose botanical species and name we wish to ascertain. Dichotomy is here employed at almost every step. We are set to determine such questions as, whether the plant has a true flower or not; if it has, whether it has both calyx and corolla or not, and so forth. This bifurcate alternative is purposely made to confront us almost every time, and is continued until at length the one finally admissible alternative comprises the species of which we are in search. But to confound such a preliminary process as this with a final scientific classification is to confound the scaffolding with the building. The path by which the novice proceeds in determining the plant is a very different thing from that systematic arrangement which the botanist adopts as his final result. Not one of the dichotomies by which we thus guide our way need correspond to any of the various orders or families which form the higher classes: in fact we shall find ourselves continually cutting across natural and fundamental units of arrangement. We employ for convenience a dichotomous path, but we have no intention of retaining a dichotomous result in the end.

III. *Rules for Division.* The technical rules for Division correspond in several respects, allowing for the different subject-matter, to those given for Definition. They are thus given in most of the manuals:

(1) The dividing members are to exhaust the whole class.

(2) The divided class must be wider than each of the divisions, and equal to them all collectively.

(3) The dividing members must be distinct and disparate.

(4) The division must be orderly, and each step must as far as possible be a proximate one: that is, we should not, at a single step, spring to a subdivision so remote that it would better have been reached by several steps.

The reader will easily see that the above rules represent a compromise between the formal and the material, as is indeed the case throughout nearly all the treatment of Logic by the later scholastic writers. They leave moreover a good deal to be desired in the way of clearness and terseness. Thus the first three might surely better be summed up in the statement that the subdivisions of any class must be mutually exclusive and collectively exhaustive.

The substance of the third rule is often given in the form of a caution to avoid *cross-division*. By cross-division is meant any arrangement by which two or more of the subdivisions run across each other, so that some of the objects fall into more than one of the classes. In a bifurcate division such a fault is not likely to be committed, for the two subdivisions will most probably be obviously contradictory of each other, either formally or materially; but when we are concerned with several subdivisions we must be on our guard here. It is said (Fowler, p. 60) that cross-division arises from the adoption of more than one *fundamentum divisionis*, but this statement seems to need some explanation. The fundamentum divisionis is nothing else than the attribute which furnishes the ground of distinction between the members of the class. Now where we are making only two subdivisions we can effect this by resort to one attribute only, namely by taking it or leaving it; and so obtaining two contradictory classes. But where we are making more than two subdivisions we must resort to more than one attribute to effect this; in other words, we must admit more than one fundamentum divisionis. What is really meant by the caution in question is rather a postulate of practical good sense than a formal rule. Three or more subdivisions will generally be found to be produced by the plan of starting with some

important attribute, and, instead of simply taking or leaving it, endeavouring to find a series of slight modifications of it. And in doing so we are recommended to be careful that these modifications result in mutually exclusive classes. Thus if we were to divide 'graduates' into those who are of Cambridge and those who are not, this arrangement must be exhaustive and mutually exclusive. But if we divide them into those who are of Cambridge, of Oxford, and of London, we might find that some were reckoned twice over, and some three times.

CHAPTER XIII.

DIVISION, CONTINUED: CLASSIFICATION.

THE traditional logical process of Division, as we saw, did not lead to much result. Indeed, in a treatise on Induction, it was somewhat of a departure from strict consistency to touch upon such a procedure at all. It was however desirable to do this, partly because the continuity of technical language in the subject rendered it necessary to give some explanation of the original meaning of certain terms still in use; and partly also because by seeing what were the main deficiencies of the old treatment, we may be better able to see where improvement is needed.

I. When we lay aside the old restraints imposed by principle and tradition, we may see our way to two very different developments, both of which may be considered to take their start from the customary logical conception of Division. The following out of that conception, as we saw, was hampered in execution owing to the fact that though the process professed to be, and really aimed at being, formal, it nevertheless attended sufficiently to the dictates of common sense to endeavour to render itself practically useful. This compromise naturally acted as a check. What we now propose to do is to see what comes of the attempt to develop these two sides,—the formal and the material,—separately, so that each shall be as little hampered as possible by the other.

(1) The first of these developments opens out to us the field of what may be called Symbolic Logic. This is an extension of Logic to which we have already had occasion to allude more than once. As however it is still one which is extremely unfamiliar to most readers, and as it also demands a peculiar mode of treatment necessitating a constant employment of what are

commonly called 'mathematical' symbols: as, moreover, I have devoted a separate treatise to its exposition, no attempt will be made here to do more than give a sketch of the process by which the foundations of this treatment of the subject may be most readily reached.

Start then from the familiar account of Division as given by the later Aristotelians. This was, as we have seen, at every step bifurcate; branching into two contradictory subdivisions. Thus Substance was divided into corporeal and incorporeal: corporeal substance into what was complex and simple, and so on. There are two points which must be noticed in this ancient arrangement. In the first place there is not any very rigid adherence displayed to a truly formal dichotomy of the X and not-X kind. However accurately the two subdivisions may, as a matter of fact, be mutually exclusive of each other, they do not always show by their very form that they are so. And in the second place, as was pointed out in the last chapter, we do not undertake at every step to subdivide *both* the resultant classes so as to continually double their number. The process, as indicated by the technical expression 'the predicamental line', is directed towards the ultimate separation, by subdivision, of some predetermined class or individual.

The Division which we now contemplate is founded upon a thorough-going recognition of these two characteristics. It deals with nothing but formal contradictories, and it purposes to introduce every alternative of which the form will admit. Suppose that we start with a class S, and that we are concerned with three attributes which shall serve as the bases of division, viz. X, Y, and Z. We divide S into X, and not-X. We then proceed to subdivide both of these by the introduction of Y, thus obtaining four classes. Introduce the third attribute Z, and make the same division again, and we get eight resultant classes. And this we might continue doing with any number of such dividing attributes.

So far nothing has been suggested except an improvement in respect of accuracy and completeness of method. But now attention must be directed to a decided alteration in the point of view, and one which gives nearly all its significance to the method in question. Instead of regarding X and not-X and their respective subdivisions as standing for actual *classes*, we

shall regard them as standing for class-*compartments*. This makes an enormous difference. It meets for instance the objection which has been raised by formal purists against the legitimacy of this kind of division, on the ground that unless we had reason to know the relevance of the attribute X to the objects included under S,—that is, unless we had the material information that some of them did possess X and some did not, —we might be led to the absurdity of a class which was without any members to compose it. Of course this would be absurd if what we were proposing to do was to make an arrangement or classification of existing objects of any kind;—in fact 'the snakes of Iceland' have passed into a common joke;—but it is far from absurd if we are proposing an exhaustive arrangement of compartments. The occasional emptiness of any one of these, as we shall see in a moment, is just that which gives significance to our results in the practical employment of our scheme.

What we start with then, on this system of Logic, is a framework of class-compartments, the number of these being determined by the number of class-terms involved in the proposition or group of propositions which we are supposed to have before us. If the propositions involved, as is the case with the common syllogism, three terms, say X, Y, Z, we should have eight such compartments before us, viz. XYZ, XY. not-Z, X. not-Y. Z, X. not-Y. not-Z, and so on, till all the possible combinations were exhausted. Now this being so, it may be shown,—and this forms the basis of the whole Theory of Symbolic Logic,—that every significant universal[1] proposition must necessarily destroy some one or more of the possible classes; that is, it must cause some one or more of the compartments to be empty. And conversely, whatever may be the number and description of empty compartments, there must be some corresponding proposition which will unambiguously express the facts. Thus, for example, if I say, 'All X is Y', this is equivalent to saying that 'there is no X that is not Y', or in other words that there is no such class as X. not-Y. So if I say that 'All X is either Y or Z'; this is equivalent to saying that 'There is no X that

[1] The case of *particular* propositions is somewhat different. It is found that they are best explained as declaring that such compartments *are* occupied.

is neither Y nor Z', that is, that there is no such class as X . not-Y . not-Z.

A peculiar interpretation of the import of propositions is of course demanded on this system of Logic; but, granted this, the whole foundation of the system may be said to consist in this exhaustive scheme of compartments.

As this development of Logic has extremely little connection with Induction, being in fact nothing but a very broad generalization of Formal Logic, we must content ourselves here with this slight indication of its scope. The reader must however understand that this process of continued dichotomy, by the introduction of every relevant class-term, forms the entire basis of the subject. The elaborate apparatus of symbols, in great part identical with those employed in mathematics, which are introduced to express the resultant formulæ, are nothing more than convenient abbreviations of processes which are essentially logical and not mathematical in their character. It is necessary to enter this protest in the most decisive way at once, because the opposite opinion has become rather firmly rooted. So far from mathematical processes and conceptions being introduced into this development of Logic, we only make use of a selection of mathematical symbols to express logical processes and conceptions. We resort to these not from absolute necessity, but because we find them ready to hand and convenient for our purpose. We could work out our results without their aid at all, as was originally done to a considerable extent by Jevons in his *Familiar Lessons*, and has been yet more completely effected since by Dr Keynes in his *Formal Logic*. It is however, I think, convenient to help ourselves by symbolic notation if it were only for purposes of simplification. The manipulation of a multitude of class-elements,—the introduction of eight terms, remember, will yield 256 such compartments,—is almost impossible in practice without some such aid. To this may be added another advantage. It is highly desirable that there should be some alternative mode open to the student, besides that of mathematics, for acquiring some knowledge of the nature and object of a symbolic language.

(2) The extension of the old problem of Division which we have just discussed was a development of the *formal* side of the process. The one which we have now to consider proceeds

upon the opposite plan, viz. that of developing to the utmost the *material* side. What this leads us to is nothing else than the familiar problem of Classification, under which name we will now proceed to examine its nature and aims.

The main object of all Classification is obvious enough. We are supposed to have before us a miscellaneous lot of objects, and we are directed to group them into classes, or rather into a sort of hierarchy of classes. The process may be commenced, so to say, from either end. On the old plan,—as the name Division implies,—we were always supposed to start with the entire group or supreme class, and to proceed, as the phrase is, downwards; the process being one of continued subdivision. We might however just as well have proceeded in the reverse direction, arranging our classes in the upward order. Had we adopted this course, a more appropriate name would have been *Aggregation* rather than Division. In the physical process of sorting shot or gravel into a number of packets according to size, it would come to the same thing in the end whether we made use of the sieves by beginning with the finest or with the coarsest. So again, in carrying out such a system of coordinated classes, we might if we liked, proceed irregularly,—as indeed is probably the commonest practice when we are dealing with actual objects in the ordinary walks of life,—sometimes upwards and sometimes downwards, until the task is completed. The fact is that a system of Classification carried out entirely *de novo* is hardly anywhere to be found. Ages before the logician, or anyone else who deals with systems, had a hand in the matter, the necessities of common life had been at work prompting men to group the things which they observed. All names imply the recognition of groups; and in the case of some names there is, if not actually implied at any rate strongly suggested, the existence of a subordination of groups; so that at the earliest stage to which we can transfer ourselves we find that we are already in possession of a rudimentary classification. We cannot even talk or think about the things which we name without some kind of reference, direct or indirect, to a rudimentary classification.

The remarks just made do not give the slightest suggestion in the way of guiding us to any principle of Classification; on the contrary they may serve to remind us that this principle

must in some way be supplied by ourselves. The mere arrangement of one and the same lot of things into a hierarchy of successive classes can be carried out in a variety of ways which is for all practical purposes infinite. And even admitting the restrictions imposed by common sense and inherited language; in other words, taking for granted the basement set of classes as they are popularly accepted,—that is, the ultimate subdivisions as they are furnished by popular speech,—yet the number of ways in which the intermediate ones, between these and the top class, may be disposed is very great indeed.

We want then some principle to guide us. This can only be given in its details presently, after we have seen what a system of classification can effect,—for here, as in so many other cases, we can only clearly determine what to aim at after we have had some practice in shooting,—but the main outlines of what we should strive after can readily be sketched out. The general object of all classification is to keep our control over the facts by marshalling the objects in order: to know where to find a thing when it is wanted, and to economize our statements in the retention and communication of our knowledge. This is purposely phrased rather loosely, but it will serve as a starting point towards realizing the distinction between one system and another.

II. There is a distinction which has been so frequently drawn in logical and other systematic treatises, that it has already begun to make its way into popular phraseology. It is that between Natural and Artificial systems of Classification. The particular phrase in which this distinction is conveyed does not seem a very satisfactory one. Every arrangement of the kind in question is artificial in so far as it is our own voluntary procedure and not a result imposed upon us from without: it is also artificial in the sense that we are seldom or never proposing to group the actual objects themselves, but only to make an ideal arrangement of them in our own minds. On the other hand every classification ought to aim at being natural in the sense of conforming to the facts and endeavouring to be as suitable as possible to the circumstances. 'Natural', indeed, is a word which it does not need the authority of Butler to condemn as rather ambiguous in philosophical discussion.

The distinction with which we are thus concerned is better described as that between Classification intended for special purposes and that which is intended for general purposes. There must be some purpose or aim presupposed in every arrangement of the kind in question, just as there must be one for the shape and size of a tool; and the determination of this purpose at once puts its stamp upon the consequent classification. It is perfectly optional on our part to select our purpose, and the purpose may be of the most various kinds; but, as soon as we have decided upon this, one particular arrangement is suggested as being more complete and convenient, and therefore more 'natural' than any other.

A good illustration of the kind of distinction here in question is furnished by the common manuals of the Flora of any country or district. Take, for instance, that of Bentham. At the beginning of the work we find an elaborate classification, under the title of an Analytical Key, the sole guiding principle of which is so to arrange the plants that a person who has a specimen of one of these actually before him may be able to ascertain the name. At the end of the book we find a totally different arrangement,—the familiar alphabetical one,—in which the determining motive is simply that a person who knows the name may be enabled to ascertain the characteristics of the plant. But the bulk of the book is of course arranged on a third, and again totally different kind of classification, of which no more compendious description can be given than that it best subserves the general purposes of study, speculative as well as practical, of the objects in question.

Each of these arrangements is, in the strictest sense, what we must understand as a Classification. That is, the objects are not merely cast into groups, each of which separately is held together by some common name or description, but these groups are again similarly cast together into broader groups, and so on, until we come to the two or three broadest which together constitute the whole.

(1) We will now proceed to a more detailed discussion of these special kinds of classification, beginning with the Analytical Key above mentioned. The object in view here is a perfectly definite and very limited one. When using the scheme we are supposed to want just one thing, namely the

name of some plant which we have before us. Doubtless much more than a name lies behind such a search, but this is the direct and immediate want. To secure this purpose everything is arranged for the most prompt and certain recognition on the part of the amateur who has the plant in his hand. And since it is found by experience that a purely bifurcate arrangement is the least confusing in such a case, this arrangement is almost exclusively adopted.

Thus, on the scheme of Bentham, as above referred to, our first subdivision of flowering plants is into those which have the flowers compound (i.e. consisting of several florets in one common involucre) and those which have them distinct. The former are then divided into such as have a single seed, and such as are two-celled with several seeds. The latter are subdivided into those with a double perianth (i.e. possessing both calyx and corolla) and those with a single perianth. And so we proceed, always endeavouring to have before us a pair of alternatives involving the presence or absence of some obvious and decisive characteristic. Even if such a notoriously superficial botanical quality as *colour* is in any case really decisive as a guide, we appeal to it readily. Similarly with such a variable character as the *habit* of the plant (e.g. whether it be creeping or climbing; a shrub or a tree; an aquatic, or a land plant) in case this will serve to include the species we want to distinguish from others.

In such an arrangement we sometimes reach our ultimate subdivision very quickly. In the example just above, the *Jasione* genus is separated off at the second step, and as, so far as the British flora is concerned, the genus contains but a single species (popularly known as *Sheep's-bit*), this ultimate class is reached almost instantly. Sometimes, on the other hand, we may have to go past fifty or a hundred of these branching sign posts before we reach our destination. If therefore we were to arrange all the alternatives, from first to last, after the fashion of a genealogical tree, we should find that the scheme would correspond in appearance to one in which certain family branches ceased at the second generation; others went on (with always two to each family) for three or four generations; whilst others gradually multiplied and broadened out so as to include an extensive set of descendants.

Definition and Division are so intimately connected that we may digress for a moment to consider what aspect is assumed by one of these artificial keys when its application is transferred from the latter to the former. Reverse then the descending process as indicated above, and at the same time fix the attention on the connotation rather than the denotation. That is, starting with the lowest class, add up all the distinguishing attributes which we had successively appealed to in order to secure its ultimate separation. What do we thus obtain? We obtain what may be called a *Definition* of that lowest class, for we have drawn out what may be regarded as the sum-total of its connotation so far as that particular system of classification is concerned. Of course it would be an extremely artificial and far fetched Definition,—and would in fact be much better called a Description, since we can hardly consider that the meaning of the term could be stretched so as to include all these attributes. But for the particular purpose in hand it resembles a Definition, though it would of course embrace a curiously heterogeneous group of attributes. The practised botanist would see at once that some of these attributes were really important in themselves, that is, are correlated with many others, and prevail in such a connexion over wide areas; whilst others are very superficial and would not continue to apply if we attempted to extend our classification beyond the country or district for which it was originally drawn up. But, regarded as a Definition, it would represent the sum-total of the attributes by which the class in question, *on that scheme*, was distinguished from all other classes.

Such an Analytical Key, as is thus furnished in any ordinary *Flora*, may be taken as a typical instance of what is commonly meant by an Artificial Classification, or, as it is better to call it, of a Classification for some special purpose. Certainly no other arrangement can be more suitable for the end in view, or therefore, in the familiar sense of the word, more 'natural' under the circumstances contemplated. The reader must however carefully notice that this artificial or special arrangement is limited entirely to the *intermediate* stages of the classification. The primary, or most extensive, class with which we start, viz. that of plants in general; and the various ultimate subdivisions or species with which we end, namely the Sheep's-bit, Wood

anemone, and so forth; must be equally recognized upon every system, and are in no case interfered with or broken up. This is merely one more illustration of what has so often been pressed upon our notice, namely that the logician, in common with every other systematist, finds a large portion of his task in the treatment of phenomena already done to hand for him by the observation and the unconscious inference of ordinary persons. Much of what has thus been done, ages before he ever set to work, he is bound to respect and preserve. Hence, in the case before us, the range of what is artificial or special is confined to the process of grouping the intermediate classes. The proposer of any new arrangement never ventures to break up the *species*, or familiar lower classes: he seldom if ever disturbs the boundaries of the *genera*. What he does meddle with is the orders or families. For them he substitutes other arrangements which are so entirely devised for the purpose in hand that they could scarcely subserve any other purpose, and therefore the resulting arrangement, as regards these, is a thoroughly special one.

(2) Another typical instance of a special classification is the familiar alphabetical one. This, like the last, must start from a commonly recognized superior group which assigns our starting point, and must end with the commonly recognized lower species. Between these limits however it proceeds on its own peculiar course, which is a very different one from any other. It is, in one respect, decidedly more artificial than the one last considered, in that the characters in accordance with which we classify are not found at first hand in the objects themselves, but exist only in our conventional signs for those objects. Consequently, since these signs,—the familiar names of the plants, if we are making a list of popular names,—vary from people to people, such a system of classification is not only special to its own subject and its own aim, but is also special to the inhabitants of the country for which it is drawn up. To enable an Englishman and a Frenchman to employ the same Analytical Key we should only need to translate it, but not to interfere with its order or arrangement. But as regards an alphabetical list of popular names, when it was translated from one language into another, nearly all signs of classification would have been destroyed.

This alphabetical arrangement is so familiar to every one that scarcely anything need be said about it beyond insisting that it as much deserves the name of classification[1] as any other arrangement: in fact it carries out the ideal of the hierarchical disposition of the classes involved more completely than almost any other. Its obvious merit consists in the extreme celerity with which it isolates the element we are in search of, the alternatives being 24 at every step, and all but one being instantly laid aside. The formal process of thus determining a class is the same as in the case last considered; but whereas, with a group of a million of things to start with, we might need 20 successive dichotomies to reach the particular object desired, we could (provided of course all letters occurred with equal frequency) get at it in *five* alphabetical selections. In practice all letters are not equally frequent, but the choice is generally sufficiently varied at each step to enable any one very rapidly to attain what he is in search of when the objects before us are marshalled in alphabetical order. To which may be added the fact that this plan can be carried out to the end, whereas most other schemes of artificial classification fail at a certain point, and then hand us over to the alphabetical method for the rest of our course. Suppose, to take a simple example, that there was a man named Timothy Hepburn, a second-hand bookseller, who lived at 27, Baker Street, and that we wanted to look him out in a Directory. We should find three different schemes open to us. We might take the alphabetical classification, and thus identify him in a few moments. Or we might determine his place by the classification of employments, beginning first with the general division of trades, reducing this to booksellers, and then to second-hand dealers. But there we should come to a stop, for the classification probably goes no further; and if we wanted to identify an individual, we should have after that to fall back upon the alphabetical arrangement in which the members of each final division are disposed.

The foregoing examples represent the extreme type of the special or artificial classification. Obvious as their nature and use must be to every one, we shall find it really serviceable to

[1] Where, as in this case and in the chronological arrangement, the things can be placed *lineally*, we more often speak of the arrangement as an *order* than as a classification.

have spent a few moments in recalling what exactly it is at which we ought to aim in these simple cases, before proceeding to such as are more complex. The sole object which we have in view is the ready and accurate identification of an individual. For this purpose something must be known about him. It may be that we know his name; and then his reference to his place in the classification, in other words his identification, is so easy and rapid that we are apt to forget that it was a case of classification at all. If we do not know his name we must fall back upon other characteristics; and the whole art of arrangement, or the superiority of one scheme over another, consists in the choice of such attributes as are obvious, permanent, and decisive.

III. The next kind of classification to be considered is of a somewhat less simple kind. Our best introduction to it will be by calling attention to a distinction which is often apt to be overlooked. When we speak about arranging things in their place in a classification, do we mean that we are actually locating the things themselves, or do we only refer to an arrangement in our own minds or in our catalogues? We almost always mean nothing more than the latter, because it can hardly be possible that the objects themselves should be so easily procurable or so permanently retainable, that they can be actually put in their places side by side. Still there are cases where we can do this, as for instance in a Zoological Garden or Horticultural Museum, and it will be instructive to notice an instance of this sort as guiding us to the kind of arrangement commonly called a Natural Classification.

Consider then the case of a large general library. There is of course an alphabetical classification of its contents. In this we only deal with the things at second hand, and when we talk of classifying them we refer to our own ideal or paper scheme. If the catalogue were destroyed, the classification would ipso facto disappear. But besides this there is also pretty certainly some arrangement of the books themselves. For the most part they are not stuck in at random, or simply according to date or size, but there will be some trace of a purposive arrangement.

Now so far as this arrangement exists, what does it aim at? Nothing more than to facilitate study. What we find is a certain amount of grouping of the same sort of subject. Books

on History are mostly in the same room or book-case: those on Philosophy, Medicine, &c. are found standing to some extent in each other's company. The object of this is to save the student's time, and the better to enable him to light upon books which he might otherwise altogether overlook. If all the political pamphlets issued during the Commonwealth are placed side by side, and a reader goes to consult one of them he is presumably the very man who will also want to look at the others. He will thus light upon many which he would not otherwise have heard of, and he will save time about those which he has heard of, by not being obliged to wander from room to room to procure them.

An arrangement of this kind, to a partial extent, probably exists in every library. The convenience experienced thereby has led to repeated attempts to carry out the scheme more fully; and so far as the mere physical arrangement is concerned there can be no doubt of its advantages, at any rate in libraries which are promiscuously attended. But when it is proposed to make a complete classification in the secondary sense,—that is, a paper catalogue or arrangement,—the question becomes more doubtful. This has indeed been attempted on a tolerably large scale,—for instance, in the extensive catalogues in the MSS. Department of the British Museum, in a catalogue raisonné of the printed books at Queens' College, Cambridge, and elsewhere. The opinion of practised librarians is however somewhat adverse to such schemes as this, and is decidedly in favour of a purely alphabetical arrangement in accordance with the names of the authors. One reason for this is simply to be found in the conciseness and rapidity of consultation on the latter plan; but another reason is more important as bearing on the theory of classifications of the kind which we have next to consider. The difficulty consists in the frequent occurrence of what are called cross-divisions, and in the want of any settled principle to decide which of the alternatives is to be preferred. We have a place assigned to works, say, on History, and another to works on Philosophy: where are we to place a book which treats of the Philosophy of History, or of the History of Philosophy? If space and money were inexhaustible we should, of course, place a copy in each division; and similarly, in the printed or written catalogue, the name should occur twice over.

But having regard to ordinary conditions of convenience, we are forced to confine our entry to one or other of these alternatives. Of course we could come to a decision on this point, and if scientific interests were concerned in the enquiry we should do so, but for the immediate purpose in hand such a difficulty is fatal. Any catalogue which requires, of the average people who consult it, a reference to first principles of arrangement, necessarily stands condemned as impracticable.

Such a case as that just discussed seems a somewhat intermediate one, and it shows two distinctions when compared with the former. For one thing, instead of having only one perfectly definite aim, namely that of identifying an individual or group, we have a somewhat general aim; that of facilitating ordinary study. This is plain enough, but a consequence of this is not quite so obvious. It results that the character and importance of all the intermediate classes between the highest and the lowest, those in fact which constitute the main bulk of the classification, are entirely altered. In the case of the special classification the intermediate classes exist only as a means to an end; we seldom or never want to refer to them for their own sakes, but only as a help to the identification of the ultimate classes. We have not the slightest interest, for instance, in the names which begin with an *S*, taken as a whole: we just rest on this as on a sort of landing for an instant, on our way towards reaching Smith or Scott or Sykes. Similarly with the highly artificial intermediate classes in the Analytical Key: "trailing plants with evergreen leaves" is one element of a disjunctive alternative which happens to make itself useful at one of the steps in the course of deciding between the *Periwinkle* and other plants, but as a class for any other purpose it is never recognized. But directly we begin to broaden the ultimate aim of our arrangement, the substantive importance and independence of the intermediate classes begin to make themselves felt. Under the general head of History there will be a department of Topographical History, and, more narrowly limited, the history of London. And each of these represents a group which has some value in itself and not merely as a means to those beyond it. Of course some persons will simply turn to 'London' as they turn, in the alphabetical list, to the letter *L*, but there are others to whom that group as

a whole represents the range of their study and interest. This we may regard as being one of the most important formal characteristics which distinguish the general from the special arrangements or classifications. It deserves notice that Linnæus himself expressly calls attention to this distinction,—on his own system of course,—between the genera and species on one hand, and the orders and classes on the other, saying that whereas the former are natural, the latter are artificial.

IV. Now conceive that the change thus indicated is carried out to the utmost. Or, to vary the mode of expression, let us state the problem in the following way. A special classification, being conditioned by this or that object, or the wants of this or that class of persons, is in a measure a personal or subjective one: let us therefore aim at a truly objective classification, which shall hold good for all persons, and which shall therefore deserve to be called a *natural* one.

There are two very different ways in which it has been attempted to carry out such a design as this; one of these being of a positive or matter of fact nature, and the other rather apt, as sometimes understood, to introduce speculative or fanciful considerations.

(1) The first of these schemes sets before itself the design of so arranging the things that we shall be enabled to make a maximum amount of aggregate assertion with a minimum number of propositions. Revert to the Directory. An alphabetical arrangement gives a minimum of such statement. The people whose names begin with *S* (unless we appeal to the refinements yielded by taking averages, and consider the nationality indicated by names) have probably nothing whatever in common beyond that fact itself. The arrangement according to *streets* marks an improvement in this respect; for many common affirmations and denials can be conveyed on this basis. All the inhabitants of such and such a street have a large income; those of another have a south aspect, and so forth. A *trade* arrangement secures the same end still more completely. When, for instance, the brewers are all placed together we find that they share other characteristics besides that of making beer, and that the direct and indirect consequences of their common employment are much more numerous and far-reaching than are those of living in the same locality.

They will probably prosper or fail under the same general circumstances: they will regard in a similar way any proposed changes in the malt-tax or the licensing laws: and so on.

There is no doubt therefore that something can be effected in this way; that is, that certain plans of grouping a number of objects will show an advantage over others in respect of economy of assertion. And this has been explicitly claimed as the ground and test of scientific classification. There can however, I think, be no question that such a claim greatly overrates the value of this principle. That we may, with more or less completeness, judge the value of a classification on these considerations, seems true; but as soon as we make this the explicit aim to be set before us, a number of difficulties present themselves. When we attempt anything beyond the rudest estimate of the number of attributes contained in an object, or the consequent number predicable of a class, we meet with the difficulty already mentioned in former chapters, namely, that attributes are not things which can be counted like apples on a tree. They are rather our ways of viewing and discussing things, and the extent to which they admit of subdivision and enumeration is largely optional. When, to meet this difficulty, it is said that we must lay the stress upon the important attributes, we only raise fresh questions; for the importance of anything is a relative term indicating the extent to which it subserves some object which we have in view. And if it is attempted to determine the importance objectively, by estimating the number of dependent attributes which are implied, we not only encounter the difficulty just mentioned, but we also find sometimes (as in mathematics) that this would not afford a clue, inasmuch as many attributes may be mutually deducible from each other.

(2) Numerical considerations of the above kind have however never been really operative, beyond giving ground for a slight preference of one scheme over another. What those who have spoken in favour of a "Natural" System of Classification have presumably had in view was rather what they would have called by the somewhat vague term "natural affinities". They considered themselves to be in some way following Nature in their scheme of arrangement, and to be making their dispo-

sitions in such a way that the things which should stand nearest each other in their scheme should be those which were actually most closely related.

These expressions, and others of the same kind, are obviously rather vague, and have a tendency to be little more than allegorical. The speculations of the last fifty years however have thrown a good deal of light upon the sense in which they may be now interpreted. It will therefore be advisable to divide our discussion by considering first what some of the older writers who used these expressions may be supposed to have understood by them, and secondly what we may now set before us when we aim at a Natural System of Classification.

The so-called Natural Classification was first worked out practically by some of the French systematic botanists. The system of Linnæus, in contrast with which the expression was used, was an intermediate one, and was avowedly offered as a temporary one by that great naturalist. To regard it as 'artificial' in the sense in which we have seen that an alphabetical arrangement is so, would of course be absurd. His system may rather be compared with one of the type of the Analytical Key of Bentham and others, being somewhat between this and the scheme of De Candolle and Jussieu. It did not, like the former, aim at nothing else than subserving the process of identification; but that it does actually subserve this purpose has always been felt to be its strong recommendation, and it is sometimes conveniently employed for this purpose even in treatises founded on the more Natural System.

(i) We may take Mill as the typical logical exponent of the Natural System under its former interpretation. He rests the scheme mainly on the doctrine of Natural Kinds, in other words on the assumption of the fixity and sharpness of separation of the various species found in nature. To illustrate the scheme take the science of Botany, and compare the results with those yielded by such a special arrangement as, say, the Analytical Key already noticed. In each case we start of course with the same primary class or natural group, namely that of plants in general; and we also reach our ultimate destination in the same final classes, viz. the various commonly recognized species. The intermediate partitions are those in which the difference is displayed. In the special arrangement,

as we saw, there was often only a single distinguishing attribute selected at each step in order to part the alternatives: our work in fact was best and most neatly done when we could thus trust to one alone. But of course when we ask, not, what is required for this or that purpose by us, but what exists in the facts irrespective of our wants, then we may find that in certain cases by choosing one attribute we have secured a number of others as well. If, that is, we had made the distinction turn upon *colour*, we should probably find that there was nothing else thrown in along with this; but if we made it turn upon the much less obvious and simple characteristic of the structure of the seed, we should find that a quantity of other attributes were simultaneously secured.

Now where the phenomena present this latter characteristic it is obvious that we are provided at once with a series, so to say, of natural breaks of continuity in the arrangement of the things: we have, presented to us, a series of classes made by nature instead of having to construct them for ourselves. According to Mill's well-known analogy, what we find to be the boundary in such cases as these is not so much a shallow trench which we can dig for ourselves but an apparently bottomless crevasse which has been placed where we find it by nature. It is in fact maintained that these natural classes are marked off from each other by an unknown multitude of distinguishing characteristics. Of course the recognition of such a state of things as this puts its stamp upon our whole arrangement. If there really are these natural breaks in the continuity of organic forms it would be as absurd not to base our classifications upon them as it would be to try to carry out geographical and political distinctions whilst ignoring the existence of rivers and seas and mountain chains.

When we come to make use of this scheme in practice, we find that it involves a slight modification of the old scholastic doctrine of Genus and Species, the familiar names being retained but a somewhat different interpretation being placed upon them. We saw that the traditional mode of Definition was that by genus and difference; that is, that we took for granted the connotation of the next highest genus, and then added on the surplus connotation or 'differentia' of the species. The so-called Binary Nomenclature of the botanists is nothing

but a slight modification of this. Ask what is the name of any plant and the botanist or gardener will give you a double name, the first part of this being the genus and the latter the specific distinction. He will say that the plant is Anemone Pulsatilla or Bellis perennis.

The distinction between the modern scientific plan and the old scholastic doctrine mainly consists in the following points. For one thing the terms genus and species were always to some extent merely relative terms; the same class might under varying circumstances become either one or the other; and modern logicians recognized still more completely the essentially relative character of these terms. On the other hand the systematic naturalist has gone in the opposite direction, and has tied down the signification of these two terms in a very strict way. With him they both always mark certain stages in the complete hierarchy of the classes. The lowest 'natural kind' discoverable is considered a *species*, the next one above this, which includes it, is called a *genus*. To mark the higher successive divisions new technical names are introduced; thus above the genus comes the 'natural order'. The total number of stages in this hierarchy is comparatively very small. Another minor point of departure from the old conception is to be found in the trivial or insignificant character of the specific distinction. In the common Logic the 'difference' is always a significant name like that of the genus; in Systematic Botany it is generally expressly stated that this has no significance, that whatever may be its apparent meaning or its actual etymology we are to regard it as a mere unmeaning label. Thus 'perennis' as an adjunct does not *mean* that the daisy in question lives for several years, nor has Pulsatilla the interpretation which the name may once have implied.

When we compare this conception of a Natural Classification, as being one which is founded on the recognition of natural kinds, with that last considered, which so made its divisions as to secure the concurrence of a maximum number of common attributes, we do not find them necessarily to concur. The botanist, or other student of any branch of science, wants of course to have some *test* of a natural kind. Fully admitting that each kind corresponds to a 'break' of an important character, yet we want some means of knowing where

these occur. The commonly accepted test is the *fixity*[1] of the distinctions. Doubtless these natural kinds would never have come to be recognized at all unless the differences involved had been many and striking in most cases, but when they have once come to be recognized their existence is decided by another characteristic,—this of fixity,—which will in certain instances be by no means coincident in result. Searching then for some simple practical test the systematic botanist has decided that when any class of things 'breeds true', that is, when it is distinguished from adjacent or resembling classes by constant characteristics, its specific character shall be admitted however small the distinction may be. It is hardly possible to mention any distinction, however apparently trivial, which, if it could be shown to be constantly characteristic of a class for generation after generation, would not be recognized by most systematists as giving ground for the establishment of a separate species. And on the other hand, however wide may be the differences between one individual and another, and however locally persistent and distinct they may seem, yet if we can continue to interpose a succession of individuals between the extreme members they would all alike be considered to belong to one species. Thus Bentham, following Linnæus, regards the primrose, cowslip, and oxlip, as mere varieties of a single species; but he retains separate, as genuine species, several which to the eye of an untrained person would seem almost indistinguishable from each other.

Some of the exponents of these natural schemes of classification have been in the habit of using language to describe them which bordered on the mystical, or which would at any rate have been very difficult of justification in a literal sense. Sometimes they have spoken as if they considered that such a scheme was one which more nearly followed the designs of the Creator than was the case with the artificial schemes. Sometimes they have confined themselves to the more rational position that on such a system those things were placed in close

[1] The account often given, that 'species' are those classes all of whose members either are, or might be, descended from a common pair, is of course no *test* in any doubtful case; such descent is rather a matter of inference than of observation. It is worth noting that this account of 'species' corresponds with the etymology of the term *genus*.

proximity which were actually 'related to', or 'in affinity with each other. But if pressed to say what was meant by relationship and affinity they would have found the answer difficult; for, on the doctrines of those who hold the fixity and independent creation of species, affinity clearly does not mean the same thing as when we apply it to human relationship, say to cousins or to any other near or remote relations in the common sense of that term.

(ii) It need hardly however be pointed out that a vast change has been wrought in our whole way of regarding these matters by the growth of the Theory of Evolution. Relationship or affinity is now understood to mean exactly the same thing in the case of species, as it does in the case of individuals. Formerly it was applied to the latter in its simple or primary sense, but to the former in a metaphorical sense. There was very commonly incorporated into the definition of a species the statement that it was composed of those individuals which were, or might be supposed to have been, descended from some single pair of ancestors,—the latter clause being presumably introduced so as to admit of a first creation of a plurality of specimens of a species. But the very same people who gave this account of relationship within the species, denied that one species had any such links of common parentage with others[1].

As soon as Zoologists came to be inspired with the Theory of Evolution their conceptions of the nature and functions of classification naturally underwent some alteration, and it becomes important to enquire to what extent this influence prevailed. Broadly speaking it seems to me that as regards the details of actual classification very little change has been introduced: that is, there has been very little breaking up of formerly established classes, or aggregation of such as were distinct. Where the consequences of the new Theory have displayed themselves has rather been in such directions as the following.

For one thing, there is an enormous increase in historic interest, and in what may be described as the opportunity of ancestral investigation. There is the introduction, so to say, of a third dimension into our subject. Existing classes could be

[1] "Species tot sunt quot diversas formas ab initio produxit Infinitum Ens" (Linnæus, *Genera Plantarum*).

symbolized, and their affinities indicated, on a superficial diagram; but if we are to take account of the past, and to represent the connection of the individuals and classes which are now extinct with all those which can be proved to have arisen as modifications of them, we should need a solid figure as a suitable representation[1]. There is not merely an addition to the range of classification, therefore, introduced by the inclusion of the past, as if some new country had been discovered, but what is thus added from the past has many and intimate links of connection with the present. It may contain the key to unlock many existent mysteries. We should indeed require a whole volume if we proposed to discuss the ways in which historic explanation has cast a light on many difficult problems of the present.

The conversion of metaphor into fact is, of course, little more than one aspect of the historic explanation. 'Affinity' now means exactly the same sort of thing when applied to species as it does when applied to ordinary human relationship. The ideal classification therefore of any extensive group of objects would be on the model of a Genealogical Tree.

Again; one of the most important changes, in the way of details, introduced by the new conception is to be found in the interpretation which we are thereby enabled to place upon the 'importance' of characteristic attributes. Attention has been already directed to the difficulty commonly experienced under

[1] Professor Flower (*Introduction to the Osteology of the Mammalia*) gives a diagram to illustrate the general mutual relation of a number of different species, supposed to be at present distinct from each other, but to be connected by affinities more or less close. The diagram rather resembles a map of a number of lakes of various sizes and shapes, and lying in different degrees of proximity to each other. This represents, of course, the now existent state of things, where we find discrete species separated by wider and narrower gaps. He then adds that "if all known extinct forms were inserted many of the intervals between the border lines of the groups would be filled up". One would have thought that the most appropriate illustration of the introduction of the additional, or *time* element, would have been afforded by regarding each of these separate lake-outlines as being the base of a sort of cone standing on the diagram. As we traced these cones upwards from the surface we should find the adjacent ones continually merging into each other, so that successive horizontal sections displayed fewer and fewer species. Each such succession would then represent the mutual relation of the species at some prescribed epoch. If we supposed that all life originated from one primordial form we should have to make all these cones spring ultimately from one common vertex.

this head, and to the attempts which have been made to solve it by considering *fruitfulness*, in the sense of possessing many derivative attributes, as a test of importance. Here the Theory of Evolution comes to our help with a more distinct and definite explanation. One element of importance, at any rate, in any attribute, is to be found in such characteristics as tell of descent. This leads to results widely different from the popular impression of importance: in fact in many cases the popular impression would be nearly reversed. For instance the somewhat paradoxical consequence follows that characteristics which are of vital importance to the welfare of the individual are, in certain respects, just those which are of least significance to the student. That is, if the race is able to perpetuate itself under seriously altered conditions, it *must* adapt its own organization (whatever precise theory, Darwinian or otherwise, be adopted as to the agency of such adaptation) in accordance with the altered circumstances. Consequently, attributes which are important for the health and vitality of the individual do not so much indicate the line of ancestral descent, as the average conditions of past life in comparatively recent times; whereas, on the other hand, qualities which are extremely insignificant, so far as the health and safety of the organism are concerned, may be perpetuated unaltered for many successive generations. Doubtless it is maintained by most evolutionists that these insignificant qualities will tend at length to disappear, but they also admit that enormous periods of time may be demanded before the disappearance is completely effected. Take but one simple instance. The systematists of fifty years ago had already recognized that *habit* in a plant was but a poor guide: that the practice of climbing and twining, and all the attributes involved in this, were not to be taken much account of in the determination of species. *How* insignificant this characteristic is, and the reason of this, can however only be fully appreciated by those who bear in mind that striking as the attribute appears to the senses, and many as are the consequences which it entails, it is nevertheless one which is largely dependent upon external conditions. So far from its being a fixed and distinctive attribute of large classes of plants, it is found that a very large number of natural orders can produce a climbing plant: even ferns can climb almost as successfully as an ivy when they

find that they really have to do it in order to survive in the struggle for life.

In the foregoing sections it is not intended to imply that the modern zoologist consciously and avowedly makes his scheme of classification a genealogical one. There have been, I believe, but few attempts of this kind. But that considerations based on this conception have had much influence in directing the efforts of the systematizers and inspiring them with their zeal seems indisputable. They would hardly have so lavished their labour had they not felt that they were engaged, if not in detecting the Designs of Nature at least in tracing out her processes.

V. So much as to Classification within the domain of the Natural Sciences. Except in that department it would not be easy to find any elaborate attempt at classification, but such as there are seem to show a great contrast in respect of seriousness of purpose in comparison with the former. Their authors do not agree, and do not expect to agree, with each other. What they give is nothing more than either a Schedule of their own mode of treating their subject, or some kind of Index or Analytical Key, of the kind previously discussed. The mere motive, in fact, of endeavouring so to arrange the objects that a maximum amount of general statements may be made about them, without any reference to something resembling a Plan of Nature in the background, does not seem sufficient to induce men to take very much trouble in striving after a perfect arrangement.

Amongst the very few schemes of this sort, to be found in other branches of enquiry, which in any way resemble the classifications of the Natural Sciences in respect of the number and variety of the objects included, is one of *Heraldry*[1]. It will be instructive to spend a few moments in considering this by way of contrast. In Papworth's *Ordinary of British Armory* will be found an attempt to arrange some 50,000 different coats of arms,—a number much surpassing that with which, say, the systematic botanist of any single country in Europe has to deal. Here, if anywhere, would be the field for a classifier

[1] We shall have hereafter (v. Chap. XXIII.) to take notice of one completer still in its scope, viz. the classifications of all nameable things whatever, as attempted by Wilkins and other compilers of Universal Languages.

so to group the objects as to secure that each class shall contain the maximum number of distinct characters; for the numbers are large enough to give much scope to the superiority of one arrangement over another, and the interest of those who do study the subject is quite keen enough to make them desire to extract most of what is to be found there. But a reference to such a work shows at once that there is, so to say, no heart in the task. The difference between attempting to arrange a multitude of these purely artificial creations, and dealing in the same way with the groups of things furnished by nature, is like the difference between trying experiments where human actions of the deliberate kind are involved and those in which we deal with the only partially explored possibilities of Nature. In a formal sense we are as much 'trying an experiment' when a letter is put into the Post Office at Paris directed simply 'Richmond', in order to see whether it goes to the United States, to Yorkshire, or to Surrey[1], as when we subject an insect or a plant to insufficient or unusual nutriment in order to see how it meets the occasion. But no one cares to carry out a course of such experiments as the former, because he feels sure that there are no otherwise undiscoverable mines of information and no new generalizations awaiting his investigation. So with classification. However we may define our purpose, we feel that the mere arrangement adopted, and any simplification which results from this, are of secondary importance. The ulterior purposes of the arrangement,—the aid and stimulus to fresh study,—far outweigh the immediate purposes, such as the control and employment of what has been already discovered.

As regards the Classification of Arms, in the case in question, there is not the slightest attempt to assign genera, species, or any of the other familiar groups of the natural sciences. Or rather there is no attempt to treat them as such, by that careful grouping according to affinity and that display of hierarchic order which are found so appropriate in the field of Nature. The arrangement adopted is as follows. The so-called principal blazon,—the chevron, fess, bend, or whatever it may be,—corresponds to the distinction of the Natural Order. We take

[1] Having once unintentionally tried this experiment I found that this was the order of precedence adopted.

note of this, and turn at once to the first appropriate subdivision, just as the working botanist turns to the Cruciferæ or Ranunculaceæ as his first hunting ground. But,—and here is seen the mark of artificiality,—all these main classes are arranged in merely alphabetical sequence. Having selected our 'order', the process of selecting the 'genus' comes next: this is decided by the colour of the field, *azure, or, argent,* &c.:—but again these characteristics are arranged alphabetically. The whole scheme of course is of the artificial or special character: it is compiled simply for the sake of enabling any one, who has a coat of arms before him, to determine the family name of the bearer of it. No one who consults such a scheme expects that it will open a way to new discoveries or generalizations of value. It does not lead us on to that study for general purposes which is the essential characteristic of the Natural System of Classification.

CHAPTER XIV.

INDUCTION.

BEFORE proceeding to discuss the general or special rules of Induction, we must come to some understanding as to the exact sense in which the term itself is to be used. This is important, for here as elsewhere the growth of speculation and the indisposition to be bound by traditional modes of procedure have recently introduced an extreme diversity of usage in the actual terms employed, quite apart from divergence of scientific principles in their application.

As regards the old,—by which is meant the traditional scholastic,—account of the process, there is but very little difference of opinion. It has always been held that Induction was essentially a process of generalization from particulars, and nothing more than this; and that it was divisible into two kinds, according as the observed particulars did or did not exhaust the generalization. The former of these was termed perfect, the latter imperfect Induction. Thus Zabarella[1] says that 'there is no one who does not know that Induction is a logical instrument by which from particular notions a less known universal is demonstrated; and that this is of two kinds, perfect, which concludes necessarily because it embraces all the particulars, and imperfect which does not conclude necessarily because it does not embrace all the particulars'. And the same statement, in much the same words, will be found in the great majority of the text books in use down almost to the present day.

But side by side with this general assent on the part of the systematic writers there has been for a long time a broadening current of dissatisfaction on the part of the physicists and those

[1] *De Doctrinæ Ordine Apologia*, 1594. Where the question is one as to the usage of terms, the statement of authors of great learning and wide repute is of course more valuable than that of writers of more originality of view.

who have been mainly influenced by the physicists. This has mostly taken the form of an ill-concealed or openly avowed contempt of the logical treatment of Induction, and a disposition to avoid the very name of Logic when dealing with this subject. Occasionally this feeling has shown itself in the form of a suggestion that what we want is some new Logic in the place of the old one. In recent times the plan has been adopted, by some of the more original and systematic writers on scientific evidence, of retaining the old terms and even the general title of Logic, including with this most of the mechanism of the Syllogism, but redefining some of the terms in accordance with what they regard as sounder scientific principles. The outcome of this, in the case of so comprehensive a term as Induction, has been such a variety of treatment and view that the accounts which various writers have given of the process seem at first sight to have really hardly anything in common.

Under these circumstances our best course will be to examine from the beginning what is the actual course through which the mind has to pass in performing the inductive act. And, in saying this, we must insist that the induction shall be an *original* one; for, as we shall soon see, it is in the earlier stages, such as are apt to drop out of sight in very familiar examples, that some of the principal sources of divergence of opinion commonly take their rise. We can thus hope to come to a fairly close agreement as to the nature of this complete process, the differences mainly applying to the question, not, whether certain stages exist, but rather whether they should be included in our definition of Induction.

I. Start then with the common statement of the process, and see in what directions we require to extend it in order to make it really comprehensive of all that takes place when a genuine induction is performed. The following slight modification of the customary symbolic rendering of the process seems a perfectly fair one:—'We observe a quality x in A, B, C; these being individual members of a certain class θ. From this observation we infer that the same quality x will also be found in any other member, say P, of the class, and therefore generally in all the members of the same class'. This, of course, is the so-called Imperfect Induction, the only one with which at present we are supposed to be concerned.

So stated the inferential step seems plain enough. Let us now look a little closer at the nature of this process, and the difficulties underlying it. The reader who has accepted the discussions of the introductory chapters will readily perceive in what directions the bare symbolic statement represented above will require expansion in order to fit it in with the actual circumstances of life and of progressive thought. For instance, what are the A, B, C, which enter into our formula? If we refer to the stock instance of most of the old text books; viz. that of Socrates, and his mortality as inferred from observation and record of other instances of mortality, we should have, no doubt, a case included in the formula; but it is a case in which all the difficulties of the preliminary stages have been smoothed away ages before any logician was born. We know that, generally speaking, what we mark out by the letters A, B, C, are more or less fictitious entities. That is, they are manifold groups, held together in a mental synthesis with the cohesive assistance of names, these names being either individual terms (in case we argued, as above, from such and such known persons to Socrates himself) or common names of lower generality (as if we argued that inhabitants of this and that country having died, Socrates also would die). In any case the mere reference to individuals as the basis or starting point of our induction presupposes that something has already been done, either by ourselves or by others, in order to recognize and constitute these A, B, C, as individuals.

Again; exactly the same presupposition is needed in order to group these individuals into the class θ to which we refer them. Here the arbitrary or conventional nature of the unity thus introduced is more obvious. When, for instance, we speak of "*the* class" θ, to which A, B, C, belong, it needs but a moment's consideration to recognize how largely we have here been deciding in accordance with some foregone conclusion. It has been already pointed out when we were speaking of classification, and is indeed obvious enough in itself, that there are an indefinite number of classes to any one of which it is optional for us to refer an assigned individual; and this is so even under the condition that each of these classes shall have already been recognized as such. Assign the *name* by which we propose to call him and of course the determination is ipso facto made, because

the class is assigned by the name; but clearly we have no right to assume, when an induction is originally made, that the name is already in our possession. Let the individuals A, B, C, be placed before us and Socrates by their side; it does not necessarily follow that "man" is the only appropriate connecting bond by which all these objects may be brought under one common class. Many other nets, some broader and some narrower than this, might be suggested, any one of which would be large enough to take them all into its sweep, and so to bring Socrates under the designation of being mortal. When therefore we talk of *the* class to which the individuals are referred, and up to the limits of which the induction is to be supposed to extend, all that can be intended is to point out the most natural, appropriate, or familiar class for the purpose.

Finally we may apply somewhat similiar remarks to the case of the attribute x. This also will in almost every case involve a synthesis or process of grouping, as to the nature of which, and the difficulty of carrying it out, we shall find that all the difference in the world may exist, according as we suppose that the work has been already done to hand for us or that we have to commence it de novo for ourselves. "Mortality" was recognized familiarly, and was accordingly named, ages before we were born, and accordingly any one of us can easily enough perceive the attribute, and distinguish it from the A, B, C, in which it is considered to reside.

Now see how these preliminary difficulties are likely to present themselves in such inductive inferences as we may be called on actually to perform for ourselves, or to pronounce upon when they have been performed by others. Without supposing that the whole process indicated above is ever likely to be encountered by any one person, or within a reasonable amount of experience, we may yet find that the track we should have to follow if we got off the beaten paths, was one which called for some considerable originative power. We might fall in with difficulties at any one of the following points.

(1) The class θ, to which A, B, C, belong, may be familiar to us already, but the property x may be something comparatively new. That is, these individuals may be thoroughly known to us by some of their attributes, and may have

acquired a common name based upon the recognition of these attributes, so that immediately on seeing any two or more of these objects as concrete specimens, we may spontaneously class them together and give them the name in question. But, on the other hand, the attribute x employed in the induction may be something by no means familiar to us, but may involve considerable originative power to detect and recognize it. The simplest of attributes, as the reader knows, may demand the exercise of synthetic power in order that it may be perceived amidst varying circumstances; and when we are dealing with such as are more complex and involve in themselves a large amount of inference, the task becomes more complicated. A case in point of this sort would be found in the liability to disease amongst the workmen in any particular trade, a characteristic which may long remain undetected. For instance, let A, B, C, be Sheffield grinders: in themselves a familiar and well-marked class. It had long been known that they were sickly and short-lived people, but the person who first clearly recognized the character of their symptoms,so as to bring the disease, as logicians say, under one concept, had no easy task to perform. Here the class was familiar, the attribute was something new and hitherto unrecognized. Similarly with lead-poisoning as a prevalent complaint amongst house-painters. Now such diseases as these are just the sort of attributes, remember, which, when they are popularly recognized and have acquired names, come to be marked with our symbols. We say, 'These A, B, C,... possess x, therefore, presumably all the members of the class will also possess x':—an easy enough thing to do when some one has already gone over the ground before us, and indicated the right path to take by setting up the signpost for us in the shape of the symbol x, or some equivalent word.

(2) Another case, the converse to that just discussed, often presents itself. Here the attribute x may be already tolerably familiar to us, but A, B, C,—the individuals in which it has, for the purpose of our present induction, to be recognized as existent,—have not hitherto been commonly classed together. That is, we may know A, B, C, as individuals, in many different aspects, and by many different names, but they may never have been picked out and associated together as common possessors of x, or of any other attribute, so as to have thereby acquired the

position of a conceived and named unity. It is as if we knew well enough what mortality was, and had even seen *A*, *B*, *C*, die, but had never thought of 'man' as a whole; in which case of course there would have been nothing to prompt us just to generalize the mortality over this exact range. We are so profoundly influenced by names that the reader may have some difficulty in even conceiving such a state of things, but plenty of appropriate instances might readily be suggested. For example, an unfamiliar disease and its treatment by some known drug would offer a fair case in point. Suppose a doctor who had never heard of any such disease as ague: who was ignorant, that is, of the name, and had never had occasion to group together any instances of the malady which might have been brought under his notice. Suppose now that a certain number of cases present themselves, and that, as it happens, they are all treated with quinine and are cured by it. To us such an example seems as simple as that of the mortality of man. We just say, This and that ague-patient were cured by quinine, therefore presumably any other specified person will also be so cured, and, generally, all ague-patients. But to our supposed practitioner who was without the clue afforded by the word 'ague', things stand on a far different footing. He has to select a number of what we call symptoms, every single one of which is also liable to present itself from time to time in some other diseases; and this group he has to keep apart from other symptoms, every single one of which is liable from time to time to intrude itself along with those which are permanently present and distinctive. And this permanent and distinctive group he has to retain together in his mind without, at the time, possessing the guide and support afforded by a name. When all this has been done, but not before, it is easy enough for him,—and, for that matter, easy enough for any one else,—to go on and say, 'Therefore the whole class which resemble *A*, *B*, *C*, in these particulars will, like them, be benefited by the same treatment as that which they found to be beneficial'.

(3) Again, there is another case, of which instances may be found in the history of science, which is peculiarly instructive as reminding us of the sort of difficulties with which actual discovery is beset, although our symbolical

notation of the process can scarcely be induced to take notice of their existence. This is the case in which the quality x is already familiar to us, as is also the class θ to which the observed instances A, B, C, belong; but in which this class is so enormously extensive, and in some respects so heterogeneous, that a powerful effort is needed in order to realize that x can really be generalized throughout its whole extent. One of the most memorable instances of this kind is afforded by Newton's discovery of the Law of Universal Gravitation. As we state this Law now it runs easily enough in the grooves of the same formula as the mortality of Socrates. 'This body (say a stone) and that (say a piece of wood), and these others, all fall to the earth when left free; therefore all bodies tend similarly to fall towards the earth, including amongst them the moon'. But to put the case in this way is to look at the matter from the point of view of those who merely have to repeat it in after times, not of those who originated it at the outset. To Newton himself, as to all the lesser men of his day, there was an initial obstacle across the path in the shape of an almost insuperable prejudice to be overcome. Every man knew well enough what 'weight' was, in the sense of the tendency of pieces of matter near the earth's surface to fall towards it. Nor was there anything strange in the conception of the material universe as a whole, inclusive of the heavenly as well as the earthly bodies. That is, the property x, in our inductive formula, and the whole class θ, of which A, B, C, were observed members, were both, so to say, in evidence as acquired conceptions, whether or not they possessed familiar popular names. But the obstacle which centuries of pre-scientific teaching had raised against simply extending an earthly property to heavenly bodies was found to be almost insuperable. It required no slight effort of genius and imagination on the part of him who first realized that the moon 'falls' towards the earth in precisely the same sense and according to precisely the same law as any pebble which drops to the ground.

Or take a recent case where the conflict having scarcely yet quite cleared off the field we can better realize the energy required in the assault. We allude here to the doctrine of Evolution, and in particular to its application to the Darwinian hypothesis. The time may come when the difficulty will seem

as trifling to extend the generalization of mutability from 'varieties' to 'species' as it now seems to us to extend that of gravity from 'earthly' to 'heavenly' bodies.

II. What we have so far considered are the preliminary steps which have had to be taken before the discoverer can expect to make his generalization, or which he must take practically simultaneously with making it. Under ordinary circumstances, however, there is another and equally important step to be taken after that generalization has been effected. This is the step known as Verification or Proof. About this process of verification, and the kind and degree of assurance which it is capable of affording, we shall have more to say in a future chapter. It will demand full consideration because the conception of Verification as a necessary part of a complete reasoning process is peculiarly characteristic of Induction. It is not indeed at all alien to Deduction,—take the analogous case of Arithmetic, where every one is familiar enough with the process of 'proving' his sums,—but it is almost systematically ignored in that branch of logical reasoning. At present we need only say that a mere generalization from observed instances, however carefully these instances have been selected and discriminated, must always be precarious; and that it requires to be supported in some such ways as the following.

Sometimes, as in the case of Newton's conjecture that the moon is subject to the attraction of the earth, the Verification will assume the form of precise numerical determination. When we have shown that the observed fall of the moon in a given space of time is exactly what the inductive generalization from the fall of stones and other small objects would demand, then we have 'proved' the induction. But, as Newton well showed, by laying aside his investigations for a number of years, until he had all the requisite facts in his possession, some such proof was imperatively required. Sometimes again the verification will take the form of indirectly showing that no other conclusion is possible. When we have once been put upon the track of conjecturing that x is found throughout the whole class θ, we may perhaps be able to show, from our previous knowledge, that nothing but x can be present to account for the facts. Often the verification is of an empirical or practical kind, as for instance

in the case of most medical remedies. We may leap to the tentative conclusion that quinine is a cure for ague generally, but nothing short of a long course of experiment can give us assurance that it is so. Or, again, instead of actually proving the fact by practical performance, we may be able to show, from our knowledge of the effect of the agencies involved, that such an event would certainly occur. For instance, in the case of the painters' colic or the grinders' lung disease; after we have made the inductive generalization that all the members of the classes in question are likely to suffer in the same way as the observed members did, we may perhaps verify the generalization from our knowledge of the properties of lead, or of iron dust. We may be able to show that a continued accumulation of such materials in the lungs or other organs of those exposed to them would induce the observed symptoms. In one or other of such modes as these, a verification of our induction is always desirable and often necessary.

Hence we may lay it down generally that a complete process of inductive discovery,—if we suppose such a process to commence at the point at which an original investigator must be assumed to have started, and not to terminate until a sound and cautious investigator may be supposed to regard the conclusion as proved,—must contain the three following steps:—

(1) There is first a stroke of insight or creative genius demanded in order to detect the property to be generalized, and possibly also[1] to distinguish the class over which this property is to be generalized. In really original inductions this step may be one of the highest degree of difficulty. Indeed, except in the trite examples of the text books, which mostly deal with such inductions as have either been familiar for ages or at any rate have had all these difficulties cleared out of their path, this primary requirement can scarcely ever be entirely evaded.

(2) Then follows a more formal process, namely that of generalization. But a word of caution is necessary here. It must not be supposed that this process is simply one of generalization and nothing more,—the mere statement that

[1] That is, this creative demand may present itself also as a difficulty in the process of generalization described in the next paragraph.

what holds true of A, B, C, will also hold true of all their class,—for a good deal besides this has to be taken into account. It is at this stage that we must claim a place for the so-called Methods of Inductive Enquiry, such as the Methods of Agreement, of Difference, and so forth. It is quite true that these methods do not in themselves, and necessarily, involve any reference to Induction,—their nature and functions will be fully explained in due place,—but the generalization is nevertheless always held in view whilst we resort to them. We should not take such pains to ascertain by these devices whether A is a cause of X, and, if not, what is the cause, unless we were consciously generalizing the cause so as to determine its action in other cases than the one experimented upon.

As we shall see more fully in due time, the common examples have a strong tendency to underrate the practical difficulties which beset this part of the process. It is not simple generalization, in the sense of mere extension, which we have to perform, but generalization through a judicious use of exclusions resting on observation and experiment. The gist of all the methods by which we are enabled to isolate the cause, and to determine over what limits it may safely be inferred, is one of analysis and exclusion. All this part of the process is mainly of a formal character, that is, it admits of being adequately expressed, for illustrative purposes, by symbolical notation.

(3) Thirdly, there is the final or verificatory stage. Were all our processes absolutely trustworthy such a stage as this would not be required; but being what they are it would be rash to omit this safeguard. Certainly any complete account of the whole procedure known as that of Induction is bound to include this stage. Now one kind of verification, and, for scientific or logical purposes, the most important kind, consists of a deductive process. We confirm the inferred generalization, or we may even succeed in absolutely demonstrating it, by showing that it follows from a combination of various known laws. Suppose, for instance, that any one had observed that on a number of clear nights there was a thick dew deposited on the ground, and that he had generalized this into the proposition that on *all* clear nights dew would fall. This, as it stands, would be decidedly hazardous. But we can add immensely to

the presumption in favour of such a generalization by showing that on a clear night there is rapid radiation from the earth, that the moist air is therefore in contact with a cold soil, that the air in contact with the soil will be cooled, and that cool air will not contain so much moisture as warm air. These facts, put together, amount almost to a proof of the generalization.

III. The above brief review of what is involved in the complete process of Induction may be found to throw some light upon the nature of existent differences of opinion on the topic. It seems to me that the disputes as to the definition of the Inductive process mainly arise from a neglect to distinguish between the various stages indicated above, combined of course with an underlying difference of opinion as to their relative importance and their suitability for systematic treatment.

(1) Whewell, for instance, may be regarded as a prominent example among those who take almost exclusively the point of view of the discoverer. His own tastes led him to investigation, or at least to sympathy with the investigator, quite as much as to systematization; and his careful historic study of the growth of physical science had naturally impressed him with a deep sense of the importance and difficulty of the task which a long line of scientific discoverers had gradually succeeded in working out. The reader who turns to the works of Whewell after those of Mill,—still more if he endeavour to ascertain the views of the former by a study of one of the few passages in which Mill has explicitly criticized them[1],—will find some difficulty in convinc-

[1] I allude particularly here to that most unfortunately selected topic of dispute as to whether Kepler's discovery of the elliptical orbit of Mars was or was not a true case of Induction. (Mill's *Logic* Bk. III. ch. 2.) In the first place there is one serious objection to such an example: in the fact that the orbit of Mars is *not* an ellipse. It doubtless would be one were Mars the only planet revolving round the sun; but as things are the disturbances caused by the other planets produce a sensible deflection from an elliptic path. And this is not a trifling point in the problem. To Newton, or to any one else who was arguing *deductively*, the existence of these irregularities would not directly affect the nature of the enquiry. But to Kepler they made an immense difference. He was proceeding entirely on empirical grounds. He had nothing but a series of observed positions before him, and he came to the conclusion, solely from these observations, that the path of the planet was not any kind of epicycle but an ellipse. He was hypothetically right, and this was more than he had any means for certainly knowing. The utmost he could show experimentally

g himself that the two writers have the same process in contemplation. Whewell, in fact, almost ignores the generalizing element in our inductions; not of course that he would deny its existence, but rather because he takes for granted that it would be sure to follow as a matter of course. And in the sort of examples with which he is mostly concerned there is some ground for such an assumption. In subjects such as Astronomy or Mechanics we are generally dealing to a very large extent with abstractions. If, for instance, we can show in any given instance that two forces of assigned magnitude and direction

was that the path was more nearly an ellipse than any other known or simple curve. Hence the enormous importance of the verificatory work; for Newton not only proved the statement in its true or hypothetical form, but he also explained the reason of the observed irregularities and even calculated approximately their magnitude.

But, apart from this, there are serious defects in Mill's employment of the example. He assumes that the path of the planet was observed as a continuous one: that *all* the intermediate positions in each revolution were already known: so that Kepler had really nothing more to do than to remark, 'Why, that is an ellipse'. He considers in fact, as he himself says, that the case is a parallel one with that of a navigator sailing round an island and then pronouncing it to be an island; and he actually urges against Whewell,—as though this was only another way of putting the case,—that *if* the planet had traced out a visible path in the heavens we should all admit that no induction was required. All this is beside the mark. The only facts which the example supposes Kepler to have had before him were a finite number of observed positions, and these he had somehow to fill in, and connect and extend by a continuous path. Now, as every mathematician knows, given any number of points we can conceive as many curves as we please each of which shall fulfil the condition of passing through all these points. The true path therefore was in no way given to observation in the sense that it only required to be recognized and named: it had on the contrary to be selected or guessed from amongst the infinite number of possible curves. Moreover, it may be added that, as every astronomer knows, the path is so nearly circular that, if it were displayed, no ordinary eye could detect that it was not a circle: the verdict of mere observation, however carefully applied to the path when drawn to scale, would be that it was apparently a circle, but that the sun was not quite in the centre.

The example is altogether a singularly ill-suited one to illustrate the difference between the writers in question. If it were worth going into further detail, however, it might easily be shown that 'induction', in the sense claimed by each of these disputants, was involved. Not only was the constructive or originative element demanded in a high degree (thus constituting the process an induction in Whewell's sense), but there was also that of generalization (thus constituting it induction in Mill's sense). What Kepler did was, from a finite number of observed positions to frame a rule for inferring all the intermediate unobserved positions, as well as those at any past or future time. This demanded a stroke of real Inductive genius in every sense of the term.

give a certain resultant force, the step to the conclusion, that all other similar forces must have a similar resultant, will almost inevitably be taken by every one who had followed the proof so far. In fact the generalization has probably been accomplished more or less consciously by the time that such a proposition as this has been clearly realized.

Whewell's works are comparatively not much in vogue at the present day, but the aspect of Induction which he thus emphasized is one which we certainly ought to keep in view. As regards his own exposition of it, I cannot but think that this was marred by the introduction of too much technical phraseology. Thus he distinguishes between the 'Conception' and the 'Idea' which are involved in an induction. This Conception is nothing else than a subjective mode of describing what has been repeatedly referred to as the preliminary synthetic or grouping process, demanded in every case where a name is not already provided to hand owing to the work having been thoroughly gone through by others before us. Thus, in our example about the ague, it was necessary that the practitioner should select the appropriate symptoms from amongst others with which they might be casually associated, and keep them together in his mind. Whewell's mode of describing this is by saying that the observer must 'form the conception of ague'. What he understands by the 'Idea' involved in an induction, on the other hand, seems nothing but what logicians called the Category, or rather summum genus, to which the conception in question belongs. Thus in Mechanics,—where alone this particular distinction becomes clearly applicable,—the 'Law of Inverse Squares', as descriptive of Gravitation, is the Conception, whilst *Space* is the Idea to which this conception is to be referred.

It stands to reason that those who adopt this general view, that is, who attach the principal importance to the initial stages of Induction, should rather depreciate the importance of the second or methodical stage. It is, for instance, scarcely an exaggeration of Whewell's account of the Inductive process to say of it, as in fact has been said, that it simply resolves itself into making guesses, and then justifying these guesses by subsequent deduction: always understanding of course that the guesses are assumed to be the outcome of trained observation in

the department of science in question, and that they presuppose a touch of inventive genius.

(2) So much for that aspect of Induction of which Whewell may be regarded as the typical exponent amongst English writers. When we turn to the next aspect,—taking this as correspondingly illustrated by Mill,—we find a complete inversion of the relative importance assigned to the two prior stages. Mill, for instance, so wholly omits the former stage that his criticism of those who propose to include it seems quite to miss the mark,—as witness his account of Kepler's discovery. This omission is doubtless in part attributable to the nature of his own studies and acquirements. He never was, and never professed to be, at home in physical science, and he certainly seems greatly to underrate the difficulty and importance of all that has had to be gone through before the point is reached at which formal rules can come into play. But it must be allowed that the limits which he explicitly sets before him in expounding his view of Logic are largely accountable for the omission. He did, whilst Whewell did not, entitle his work a treatise on Logic; and he expressly excludes from his province the discussion of Discovery as distinguished from Proof. The only complaint therefore that can fairly be brought against him is that he greatly underrated the intricacy and importance of a step which he did not undertake fully to discuss; this inadequate appreciation being of course one ground of such restriction.

We have mentioned Mill as the typical instance of one who has made Induction coextensive with the process of generalization, because for purposes of illustration it is best to take a recent writer and one who has so deeply influenced the course of English speculation. But the name which will naturally occur to the mind, as possessing a prior claim in this connexion, is that of Bacon. To enter into any discussion of his system would involve an historical enquiry which is quite foreign to our present purpose; but a few remarks may be made here with the object of bringing into clearer prominence what seems to be the true aim of Inductive Logic.

It has been already insisted on that the process which we understand by Generalization is to be interpreted in a wide sense: that we must be careful to associate with it all that

process of analysis, and of the requisite exclusions, by which alone, as we shall more fully see hereafter, generalization can be made trustworthy. Now this was just one of the principal things upon which Bacon insisted both in precept and in practice. Every one knows his condemnation of the old 'Inductio per simplicem enumerationem',—that is of mere summary generalization from observed particulars,—that 'it is puerile, and precarious, and exposed to danger from contradictory instances', and his repeated declaration that what is wanted is something very different from this, viz. an Induction 'which proceeds by rejections and due exclusions' (*Nov. Org.* I. 105). This conviction gives its impress to his whole system, as shown in his elaborate scheme of 'instances' with their quaint appellations, *prerogative, crucial,* &c. This was the positive and effective part of Bacon's teaching. But what is still more characteristic of it, for the purpose at present before us, is his acceptance of what may be called, for want of a better word, the *alphabetical* view of the Universe, in its extremest form. It was not that he underrated the mere complexity of nature; on the contrary he often insists how far its 'subtlety' exceeds that of our senses and intellect; but he never seemed to realize the enormous difference between working by aid of distinctions which have already been recognized and rendered familiar by the labour of others and creating these distinctions originally for ourselves. He compares his methods, for instance, to a pair of compasses which when put into the hand of any beginner will enable him to draw a perfect circle. His sketch of 'Solomon's House', where all intellectual ranks and classes were to be directly associated in the work of natural discovery; his scheme of 'vintages'; his elaborate recommendation of so many different kinds of 'instances' which were to be collected prior to the work of Induction; all point the same way. Good as they may be in their proper place, that is, in the master's hands, the pupil or beginner will not find that he can effect much by their help. The whole idea involves what we have just called the alphabetical view of the universe; in other words, that we may regard it as practically broken up, partitioned, and duly labelled in every direction; so that, enormously great as is the possible number of combinations which these elements can produce, they are nevertheless *finite* in number, and will therefore yield up their secrets

to plodding patience, if only the diligent student be supplied with proper rules.

(3) Lastly, as a representative of those who would confine the range of Induction almost entirely to the third, or deductive stage, we may fairly take Jevons. He is indeed not always quite consistent in his account of the process,—in his *Elementary Lessons*[1], for instance, his description of it does not materially, if at all, differ from that of Mill,—and where he does adopt the view in question his analysis seems to me somewhat confused. But still, on the whole, there can be no doubt that he is more committed to this view than to any other. He lays it down repeatedly and emphatically that Induction is simply an *inverse* process; the inverse of Deduction; and indeed this particular expression has come to be somewhat associated with his name. This statement in itself is perfectly explicit and intelligible, and his whole treatment of what he expressly calls "the inverse[2] or inductive problem" is consistent with this and with this only. Now though to offer this as a definition of Induction is a great mistake, the process thus indicated is a very important adjunct to true Induction; and we shall therefore find it convenient to examine it somewhat in detail under this designation.

The meaning of an 'inverse process' is well known. It presupposes a direct process with which it is connected in the following way. The direct process starts from certain data and reaches a conclusion. That is, the particular premises and the laws according to which we are to infer, are supposed to be given, whilst the conclusion is to be obtained by us. The inverse process, on the other hand, starts from a conclusion and tries back for the data. That is, the conclusion and the laws according to which we are to infer are supposed to be given, whilst the premises are to be obtained by us. All this is perfectly familiar to the mathematician, and

[1] It ought in fairness to be allowed that this work was written more as an introduction to Mill than as an independent treatise.

[2] Neither the expression nor the doctrine are peculiar to him. The doctrine, as will presently be pointed out, is only another way of stating that of Whewell, and others, that Induction is nothing else than making good scientific guesses, and showing that the desired conclusion can be deductively inferred from them. The particular expression had been already employed by Tissot (*Logique objective* p. 248) "La déduction est l'inverse de l'induction".

may be illustrated by the simplest arithmetical rules. Thus the question, What is the result of multiplying twenty-five by itself? calls for a direct process; the answer being 625. But the question, What is the number which multiplied by itself will yield 625? calls for an inverse process; the arithmetical answer, given by extracting the root, being 25. And similarly in other cases; division being the inverse of multiplication, subtraction of addition, integration of differentiation, and so on.

Between these two processes there is one distinction of primary importance. The direct process is determinate, that is, it admits of only one answer; whereas the inverse is often indeterminate, that is, it may admit of more than one answer. Even in such a simple case as the one above we may notice this distinction. The product 25×25 can only yield 625; but 625, when we take the algebraical view of the matter, can be obtained as a square from either of *two* numbers, viz. *plus* 25 or *minus* 25. Here the inverse process is indeterminate only to the extent of admitting two alternatives. In the case of integration it is indeterminate to the extent of admitting an indefinite number of answers, for the constant which is introduced into the result may have any value whatever. Nay, even in such a simple process as merely arithmetical multiplication, the indeterminate nature of an inverse operation will show itself unless we so restrict the limits of the question as purposely to exclude it. For instance, What is the product of 5 and 4? Answer, (definitely) 20. But what is 20 the product of? Answer, (indefinitely) of either 4 and 5, or of 2 and 10; or, if we admit fractions, of any number whatever of suitable factors we may choose to select.

When therefore we apply this conception of an inverse process to deductive logical rules, it becomes necessary to enquire how we are to deal with this characteristic of indefiniteness. Take a simple syllogism in *Barbara* to start with. From the premises, All *M* is *Q*, All *P* is *M*, we deduce without any ambiguity, All *P* is *Q*. Given these data and the recognized syllogistic rules, and no other conclusion can be obtained. Now the true inverse of this, on the interpretation indicated above, would be this:—Given the conclusion, All *P* is *Q*, find from what premises it was obtained. But

this is clearly indeterminate to a high degree, for the problem admits of an indefinite number of answers; since *any* middle term whatever would answer as well as M, provided its extent was intermediate between that of P and that of Q. The only way of removing this ambiguity would be by either specifying what middle term was to be employed, so as to leave only the form of the premises to be determined; or by actually assigning one premise and so leaving only the selection of the other open to us.

The arbitrariness and ambiguity of this proceeding become much more conspicuous when we take some conclusion drawn from a complicated group of premises, instead of one which, as in the syllogism, involves two only. Jevons, indeed, simplifies matters by tacitly assuming that no terms are to be introduced into the premises which were not already given in the conclusion; thus doing away with any such process of elimination as that of which a specimen was seen even in the case of *Barbara*, and which exists to a greater or less extent in almost every instance of deductive reasoning. But this is a forcible reminder how entirely this way of describing Induction is limited to the symbolical or alphabetical treatment of Logic, and how wholly inappropriate it is to such problems as present themselves in nature. If for instance we start with such an example as that discussed in the note[1], and say, Given

[1] For instance, amongst the "Inductive problems for solution by the reader" (*Principles of Science* p. 126) is the following,—I state it in my own words,—Let there be three classes A, B, C, and let it be known that the only existent combinations of them are ABC, AC. not-B, BC. not A, B not-A. not-C, "find the laws governing these combinations." His answer is (*Studies* p. 256) $A = AC$, $ab = abc$; or, in words, "All A is C, and All that is neither A nor B is not C". That is, these are the premises from which the given conclusion would follow.

This is a true inverse operation, but mark three conditions involved in such a rendering of it. (1) It is presupposed here that no term is to be introduced into the premises which is not in the conclusion. We have a perfect right to lay down what rules we please in solving puzzles, but we have not a right to compare the operation carried out under this restriction with that of "discovering the laws governing the combinations" of elements in physical enquiry, and to call them both alike "inductive problems". (2) The proposed solution is only one of several equivalent ones which might be proposed. What is being asked for, in fact, is not *the* solution, but the most summary and convenient form in which a solution can be framed. Jevons says (*Studies* p. 257) that in answer to one of his problems he obtained several answers "curiously enough

that certain combinations of A, B, and C, are the only existent ones, find a solution in terms of A, B, C, and of nothing else, from which this result shall follow; no complaint can be made. The problem is a very limited one, but it may be useful; and there is nothing to hinder us from adopting what rules we please in working out puzzles. But to make the same restriction when the problem is, Given that dew is copious on a cold clear night, or Given that a magnetic needle is deflected by an electric current, find a solution which shall introduce no fresh terms into the statement of the phenomena than those already to hand, would be a mere parody of physical investigation.

There is, no doubt, a certain inverse element in Induction, but this element forms far too small and accidental a part of the whole allied group of processes to have any right to claim the name to itself. Moreover there is the less occasion to divert the old term, so long recognized in Logic, to this new and narrow signification, because the process to which it is thus proposed to apply the term has long been known and named. It was even familiar, in a certain form, to the old logicians; for we find them occasionally applying the term 'Method' to denote the process of hunting for middle terms by aid of which a given conclusion could be proved. This is clearly a case in point of the inverse problem, above referred to, as applied to the syllogism, and its solution is obviously an indeterminate one. It was thoroughly recognized that no exact rules could be laid down for discovering such middle terms, but hints were offered and devices proposed for diminishing the labour of the discovery[1]. The Categories were largely employed for this pur-

all differing in the forms of proposition" offered as the solution. The curious thing, surely, would have been if the writers had agreed in their particular selections. (3) Not merely is the notion of generalization outside observed limits,—the so-called 'inductive leap',—omitted from notice; it is distinctly excluded. The 'premises' and the 'conclusion' are merely different renderings of one and the same set of facts, and therefore each is exactly as certain as the other. The characteristic which has been universally understood to distinguish true Induction is wanting here.

[1] See, for instance, the once well-known text book of Burgersdyck, under the heading, 'De inventione medii accommodatâ ad figuras et modas'. He gives various rules for the process, and aids them by mnemonic lines and a diagram. As regards this last it is worth remarking (what I have not seen elsewhere noticed) that the familiar expression *Pons asinorum*, now universally applied to

pose and found in this their principal use. Duhamel, again, in his *Méthodes* (I. 24) has discussed the inverse process to Deduction, and has proposed a special name for it, *Reduction*, as contrasted with Deduction and Induction. This seems to me a decided improvement in terminology, though it is perhaps best to avoid any new technical term, and to regard the process as being nothing else than the verification demanded in order to establish the Induction.

In the sense then in which we propose to use the term Induction, and which coincides as near as may be with that in which it has on the whole been commonly employed hitherto, it will be confined to what has been above described as the middle stage of the whole group of operations: viz. the stage of generalization. With this must of course be combined those safeguards for correct generalization, and those technical rules for isolating the 'cause' of an event which will be described in another chapter, the importance of which rules was first fully realized by Bacon. This group of processes all belong to the same general kind: they are comparatively definite and certain; they do not presuppose originative genius, or what those who do not themselves make the discovery are apt to call lucky guesses; but they do presuppose that the materials in hand have been already gone over, grouped into classes, and named for us by other people of a more inventive turn. They seem therefore to be an appropriate subject for logical investigation. I do not at all fall short of Whewell in my estimate of the difficulty and importance of what he calls "framing the conception"; but inasmuch as this is an act for which nothing in the nature of a *rule* can be offered for our guidance, we shall do best to exclude it from Logic. What we mostly stand in need of for our purpose are such general characteristics as a sound judgment, desire to attain to the truth, accurate observing faculties, and so forth, rather than technical rules. As regards the final stage, I shall, with Mill, consider this to consist in Verification; understanding, of course, that true deductive verification ('the inverse operation') is only one kind of

a proposition in Euclid, was formally applied to this diagram, and presumably transferred as the study of Logic died out. The diagram resembles a complicated modification of the common one for illustrating *Opposition*, and I suppose the cross in the centre of it represented the supposed bridge.

verification, and that there may be, and indeed are, other kinds as well.

IV. As a further exemplification of the nature of Induction we will conclude this chapter by briefly noticing certain modes of describing the relations which it bears to Deduction. The accounts which it is thus proposed to discuss, however, are not so much conflicting theories as to the nature of the process, as short descriptive designations of various aspects of the process which are consistent with any reasonable theory about its nature.

(1) The first of these is a metaphor; but, since nearly all our language in the province of mind is metaphorical in its origin, it is none the less deserving of notice. For instance the expressions of Bacon,—'ascending' and 'descending', respectively for Induction and Deduction,—have met with considerable favour. There is a whole vocabulary of expressions in common usage which rest upon the same analogy: thus we speak of 'rising to first principles', 'coming down to particulars', 'heights of abstraction', and so forth. And yet it is not very easy to feel quite sure as to the exact aspect of the process on which the metaphor is founded, especially when we remember that there is another class of very similar expressions which depend upon a precisely opposite comparison;—thus we talk of profound research, of digging deep for our foundations, of truth being buried or lying at the bottom of a well, and so forth. The notion conveyed by the former metaphors is presumably this. He who is in possession of a generalization is like a man on the top of a hill; not merely on account of the toil expended in getting there, for we may expend trouble as well in digging as in climbing, but on account of the increased power of insight and command of particulars which he thus acquires. To hold a generalization is, in other words, to be able to group a multitude of facts under one formula, and thus to bring them all simultaneously into relation with each other. To view a wide prospect conveys, to the bodily eye, a similar power of coordinating an infinity of details which, if visible at all, seemed to stand in no relation to each other when viewed from below.

(2) Again; it is sometimes said that in Induction we proceed from effects to causes, whereas in Deduction we proceed

from causes to effects (Fowler's *Bacon*, p. 121). This statement seems to be only partially true, and to rest upon that particular account of the causal relation which has been termed in a former chapter 'the popular scientific' account. It would not continue to hold good when the cause and the effect are both very loosely defined; and it would equally fail when they are defined with the utmost conceivable accuracy. That is, it seems to require that the relation between cause and effect should not be a simply reciprocal one. For instance, where the consequences of an illness or a wound are precise and definite and could not be produced by any other injury, our inference is equally certain, and is as naturally couched in the deductive form, whether we start from the cause to infer the effect, or conversely. On the other hand, when, in accordance with popular usage, we omit many of the antecedent elements, our argument in each case alike claims no greater force than that afforded by Analogy or Probability.

When, however, we start with the assumption that every event is preceded by some one or other of certain groups of phenomena any one of which would certainly 'cause' that event, we see that the relation between the two elements is no longer reciprocal. Given the antecedent the consequent necessarily follows, and the inference can easily be thrown into the technical deductive form:—All X is followed by Y, This is an X, therefore this will be followed by Y. But given the consequent we can only conjecture the antecedent. If we are to determine with certainty which of the possible known causes was productive in the case in question it can only be by an inductive process; that is, we must set to work by making those exclusions and employing those methods which are generally known as Inductive, and which will be described in a future chapter. Although therefore we cannot correctly say that arguing from effects to causes is Induction, it is nevertheless true that in the current logical sense of the word 'cause' this procedure will almost always demand an appeal to Inductive processes, whilst the converse procedure will not do so.

(3) There is another of these descriptions, of a somewhat epigrammatical character, which we owe to Buckle. He says

that in Induction we reason from facts to ideas, and in Deduction from ideas to facts. In so far as this is another way of intimating that Induction involves generalization, it is true enough; but like most apophthegms it sacrifices accuracy and completeness to brevity. Do we never reason from ideas to ideas, or from facts to facts? and, if so, by what correlative names are these particular processes of inference to be designated? The language here used approximates closely to some of Whewell's modes of expression, and seems open to the same criticism. What I think we may understand it to mean is this: that the law in which we sum up a generalization of a number of facts belongs to the class of subjective or mental acquisitions. Or rather,—and this is a very important qualification,—it involves more of the subjective or mental element than is involved in what are commonly called 'facts'. Of course Universal Gravitation is a 'fact', just as much as the falling of a single drop of ink into my ink-pot; it is nothing more than a compendious statement of that and every other such case of a falling body. But we may intelligibly describe the former as an 'idea', in contrast with the latter as a 'fact', if we merely mean to indicate how much more of mental synthesis is demanded in the one than in the other. For instance, were the world inhabited by brutes only we could still suppose such facts as the fall of individual bodies to be observed, and in a way to be reasoned about, but we could not suppose the same as regards any recognition of the general law of Gravitation. And since, with all due qualifications,—though these are both numerous and important,—we may say that Induction and Deduction respectively involve generalization from individual observations, and specialization to narrower, and even to individual results, from given generalizations, it is to a certain extent correct to characterize them by these designations.

The contrast however, as thus expressed, is far from exhaustive of our reasoning processes. For one thing there can be little doubt that we sometimes reason from 'facts' to 'facts'. As Mill has remarked, much of the almost unconscious inference of daily life is carried on in this fashion. And again the highest kind of reasoning would have to be described, if these appellations are to be retained, as reasoning from 'ideas' to 'ideas'. That

is, where we are dealing with long trains of reasoning, and with a somewhat abstract subject-matter, we may proceed for many steps before we come down to details at all.

(4) The above designations have been noticed here, because when we are dealing with so wide reaching a process as this of Induction, every account of it which has been sanctioned by able men is sure to direct attention to some one of its many aspects. And as regards this particular subject, the main source of confusion, as I have endeavoured to point out, has consisted in the fact that the different writers have been too much in the habit of thinking only of the side which seemed to them the most important, or with which they happened to be principally concerned.

The most familiar and important of such descriptions however is that in accordance with which Induction and Deduction are regarded as equivalent respectively to Analysis and Synthesis. This will require somewhat more careful examination, both on account of the wide-spread acceptance of this equivalence, and on account of the decidedly ambiguous signification of the terms employed to describe it.

In the first place: What exactly is meant by Analysis and Synthesis[1]? As now understood they are almost universally regarded as the processes,—mental processes, for the most part, but also extending to physical processes in a few cases where it so happens that thinking can readily be translated, step by step, into action,—by which we break up a complex whole into its parts, and put together a number of parts in order to constitute a whole. It is in this sense, for instance, that the terms are used in the only science in which they have passed into familiar technical use, viz. Chemistry. By Analysis the chemist never means anything else than dividing some compound into its constituent elements, and by Synthesis the building up of a compound out of its elements. And since his science is largely a practical one, he would understand presumably, unless otherwise stated, that the terms applied to the actual physical phenomena themselves, instead of to the mental operation of merely conceiving, with whatever clearness and certainty, how the phenomena would take place.

[1] We shall have more to say about these processes in a future chapter.

Now if we resolved to take the words in this, their currently accepted signification, what exactly should they denote in the province of Logic? Of course no contrast can be introduced here, such as exists in the case of Chemistry, between the physical operation and its mental counterpart, because our subject-matter immediately in hand does not profess to go beyond the terms or concepts with which we are dealing. Accordingly, all logical traditions of usage being laid aside, the following I apprehend would be regarded as tolerably exact cases in point. To illustrate Synthesis, we might take the two simple premises 'All *A* is *B*', 'All *A* is *C*', and combine them into the result 'All *A* is *BC*'. And to illustrate Analysis all we need do is to start with this conclusion and resolve it into the above two premises. And, more generally, the same names might be respectively applied to the processes of combining any number of separate premises into some one result, and of resolving such a result into a number of such separate premises. In fact we should employ this antithesis to express just such a pair of contrasted operations as we discussed a few pages further back, under the title of an *Inverse* process (p. 359).

Had premises and conclusions of this type been familiar to cultivators of the Common Logic, as they are to those of the Symbolic System, it can hardly be supposed that Synthesis and Analysis, if used in their present current sense, would ever have been applied to mark any other pair of operations than this. But, as the reader well knows, this way of treating propositions, that is, by resolving to retain all their significance, instead of adopting a conclusion which involves a large amount of elimination, is hardly recognized in the common system. Accordingly the current explanation and usage of these terms are not by any means so simple as might appear, and in fact there is much of ambiguity and even direct contradiction in their application.

The employment of the term Analysis to indicate Induction, probably dates, for English readers, from some well-known remarks by Newton in his *Optics*, which have been quoted by Dugald Stewart and made the text of an interesting discussion. "As in Mathematics so in Natural Philosophy, the investigation of difficult things by the method of Analysis ought ever to precede the method of Composition. This Analysis

consists in making experiments and observations, and in drawing conclusions from them by Induction,...... By this way of Analysis we may proceed from compounds to ingredients; and from motions to the forces producing them; and in general from effects to their causes......This is the method of Analysis. And the Synthesis consists in assuming the causes discovered and established as principles, and by them explaining the phenomena proceeding from them, and proving the explanations."

It is obvious that what Newton is here speaking of is something far wider than the mere logical processes indicated above. The general tone of the whole paragraph seems an inculcation of Newton's discouragement of à priori reasoning, when not founded on adequate observations, as in his well-known statement "Hypothesis non fingo". Like all good advice it must be taken in connection with the actual mental habits and dangers of the times in which it was written, and in reference to the subject-matter then and there in view. Newton, it must be remembered, was mainly concerned with subjects which admitted of mathematical demonstration, and in which therefore no less cogent proof could be allowed.

It is quite true, when we take such a wide scope as the above, that there is a large employment of Analysis in the operations referred to. But so there is of Synthesis too, and they are both employed also in Deductive reasoning. In fact a great contemporary of Newton,—Hooke,—has precisely inverted the application of the terms. He says:—"The methods of attaining a knowledge in nature may be two; either the Analytic or the Synthetic. The first is the proceeding from the causes to the effects. The second from the effects to the causes....This [the former] begins from the highest, most general and universal principles or causes of things, and branches itself out into the more particular and subordinate. The second is the more proper for experimental enquiry, which from a true information of the effect by a due process, finds out the immediate cause thereof, and so proceeds gradually to higher and more remote causes and powers effective, founding its steps upon the lowest and more immediate conclusions."

The fact seems to be that Analysis and Synthesis must be understood in a far wider sense than that in which we are able

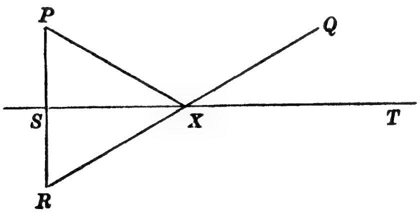
to identify them respectively with Induction and Deduction. They are processes of great generality, not confined to either of these forms of reasoning. They are, in fact, frequently applied to practical operations quite outside the range of reasoning. The utmost that we can say on this head is that, of the two, Induction decidedly makes much more use of Analysis than Deduction does.

We shall have more to say about these general processes in a future chapter, but meanwhile a few pages may conveniently be added about their employment in Mathematics, since it is generally admitted that it was from this science that they were originally borrowed for use in Logic and Natural Philosophy. According to Stewart their customary signification in Mathematics is as follows. When we start with a conclusion, supposed to be set before us for proof, whether this bear the name of a problem or a theorem, and reason 'backwards' from it until we come to some axiom, or other proposition which is admitted to be true, then we are said to reason analytically. When, on the other hand, we start from such axioms or other propositions and thus reach the problem or theorem which has been set before us, then we are said to reason synthetically. A typical instance of the former process is any 'problem' such as is proposed in examination papers: a similar instance of the latter is furnished in most of the examples in Euclid's *Elements*.

Between this usage of the terms, and that described above, there is more difference than may appear at first sight; and this difference is indicated in the accounts given respectively of their etymology. In the common interpretation, analysis is understood to be a breaking up into parts, a process of *dissolution*; and in accordance with this sense we generally find that the stress is laid upon the fact that the path backwards from the conclusion branches out, so as to lead us to a plurality of data or axioms. In the mathematical sense analysis is understood to be a *retrograde* process, that is, one from the unproved (i.e. the problem before us) to the proved (the axioms which establish the problem). Its etymology here is supposed to come, not from the breaking of a whole into pieces, but from the undoing of a knot in which we may best succeed by going back step by step along the path which any

one would have taken in the act of making the knot. Thus in proving a theorem, or solving a problem which is supposed to be set before us, we take the result provisionally for granted as a starting-point, and say; If this be true, then would that, and if that be true so would something else; and so on, until we come to some already recognized truth. The fact of being led back to this point establishes the conclusion. It is obvious that it is in this way only that we could generally expect to be able to solve any proposed problem, since we are not supposed to have any hint given to us as to what premises we had best select as our starting-point, and it would be absurd to keep on trying one after another until we had hit on such as would answer our purpose.

The logical reader may be surprised here at the implied assumption that if we can derive any true proposition from the proposition proposed to us, this latter may be at once accepted as true; since this may seem to conflict with the well-known doctrine that from false premises we may yet possibly derive a true conclusion. There is however no real conflict. The difference arises mainly from a very important characteristic of mathematics as contrasted with Logic. In the former science most of our propositions are of the nature of equations, rather than ordinary predications. Hence they are simply convertible, in the sense that if from X we obtain Y, we know that from Y we can obtain X. Take a simple example of a geometrical problem. Let it be required to find the point on a given straight line from which, if straight lines be drawn to two given points, they shall make equal angles with the given straight line.



Let P and Q be the given points, and ST the straight line. We start with the assumption that X has been found; that is,

that a point has been somehow discovered such that the angles SXP, TXQ, shall be equal.

What we then try to do is to deduce some consequence from this assumed result which shall be obvious, or in some way already known. We say, If $QXT = PXS$, then $RXS = PXS$ (for $SXR = QXT$); and if $RXS = PXS$, then $RS = PS$ (where PSR is perpendicular to ST). Then comes the simple conversion; If $PS = SR$ (a result we can readily secure by our own construction), $RXS = PXS$; and if $RXS = PXS$ then $QXT = PXS$. This process of conversion is called synthesis, whilst the previous process, of which it is the conversion, is considered as one of analysis.

This regressive process is a very common one in mathematics. It is peculiarly appropriate for solving a desired problem, or proving some proposition whose truth we can at best suspect, for thus we secure a definite starting-point. Even so we have of course to act at random to a certain extent, for we may try one path after another without finding that it leads us to any known axiom or to any obviously feasible construction. There is large opening therefore for skill and sagacity; but this procedure nevertheless involves a whole order less of vagueness than if we attempted to begin from the other end. In this latter case we should have exactly the same number of paths opening out before us whatever starting-point we selected; but in addition to this the number of such possible starting-points, in other words the number of axioms or theorems at our choice, is practically infinite. Accordingly the Analytical method, as giving us one definite starting-point, offers great advantages.

On the other hand the Synthetic, or progressive method, in which we start from axiomatic premises and derive one conclusion after another from them till at last we come to the assigned problem or theorem, is the best order for exposition. There is an obvious simplicity and naturalness in thus making sure of every step that we take from the first, instead of having the whole procedure uncertain up to the last moment.

Although however this mathematical sense of Analysis has the warrant of antiquity and long usage in its own appropriate province, it seems now to be acquiring a much vaguer

acceptation. Owing to the wide prevalence of 'analysis' in this retrogressive sense, in the domain of Algebra, *any* algebraic treatment of a subject is coming to be termed analytic. Thus those who speak of 'Analytic Geometry' probably have little in view beyond the symbolic treatment. They are not in any way contrasting their method with the Synthetic, but rather with one which depends upon intuition and geometrical construction. They would consider the word equally appropriate even though they adopted a method as closely as possible analogous to that of Euclid in respect of systematic exposition, provided only the successive steps were expressed in symbolic or algebraic form.

CHAPTER XV.

THE SYLLOGISM IN RELATION TO INDUCTION.

THE discussion in the last chapter was intended to convey a general notion of the nature of the Inductive process as here conceived. There is however one particular aspect of the question which needs more minute investigation in a systematic work on Logic. This is the relation of Induction to the ordinary Syllogism. As Mill's doctrine on this subject is familiar to most students of philosophy, and as this doctrine seems to me,—with considerable reservations and modifications,—to be tenable, we will begin with a brief statement of it and then proceed to offer some criticisms upon it.

The doctrine in question may be briefly stated as follows. All knowledge is originally derived from experience of particulars, so that every really general proposition must have been obtained by our having generalized beyond the limits of observation. Now when we examine a valid syllogism of the first figure[1] we see that it starts with a general proposition, and that this proposition actually contains the conclusion. (Thus, 'All M is Q, P is M, therefore P is Q'; where P itself is clearly nothing else than a sample of those M's of which the major premise speaks.) The question is then raised; Under these circumstances, is the process of passing from the major, by aid of the minor, to the conclusion, a begging of the question, or not? In other words, at the time that we stated

[1] It deserves notice that Mill makes no explicit reference in his discussion, to any other figure than the first, so that he must be presumed to hold that this is the only fundamental form of syllogistic reasoning: the other figures requiring to be reduced to the first. If we were to apply his description to the other figures, we should find that the functions of generalization and explanation respectively attributed to the major and minor premises would not hold; and the attempt to transfer the explanation leads to very awkward results.

that 'All *M* is *Q*', did we or did we not know that *P* was *Q*? If we did, then there was no need to go through the parade of stating two premises and pretending that there was an inference: if we did not, then we had clearly no right to state the major premise thus broadly and confidently.

Such is the difficulty. The solution is found in frankly admitting that there is no inference within the limits of the syllogistic process itself, but that the inference was secured in the act of obtaining the major premise. The true original premises were the facts which had been observed, and those only. Directly we had generalized these into our major premise we had already performed the whole inference, in that direction, which was warranted by them. This accounts for the major premise; but of course the next question is, What is the use of the syllogistic form? To this the reply is that such a form is one of considerable practical convenience. Though all inference is essentially one from particulars to particulars, it is nevertheless a great safeguard to make two distinct operations by separating the process of *recording* from that of *interpreting* our generalizations. The major premise records the inference in its widest extent: the minor interprets it by applying it to any particular cases as these arise.

Such is the Theory, stated in the briefest terms; which, it must be remembered, has found acceptance not only amongst those who are in general accord with Mill's philosophy, but also on the part of some who are strongly opposed to his general principles. For instance, such a strenuous antagonist as Whewell admits that the doctrine "that the force of the syllogism consists in an inductive assertion with an interpretation added to it, solves very happily the difficulties which baffle the other theories of the subject" (*Philosophy of Discovery*, p. 289).

Instead of proceeding directly to criticize this theory, it will be more instructive to begin by enquiring what are the grounds proposed for accepting it. These seem to be mainly the three following; two of them being positive, in the sense that they are direct inducements to accept the new theory, the third being negative, in the sense that it is supposed to constitute a difficulty in the way of our accepting the common theory.

(1) In the first place. It may be considered that this account of the syllogism is a consequence of the empirical theory as to the origin of our knowledge. This does not seem to me to be the case, at least not exclusively. Doubtless those who adopt the empirical theory would be driven to accept some such explanation as the one before us, but there is no reason why it should not also be held by those who maintain the à priori nature of a large part of our knowledge. This is, in fact, Whewell's position, who accepts Mill's account as applied to the bulk of our physical knowledge. So far as this motive is concerned, then, all we can say is that the empirical theory enables any one who accepts this explanation to regard it as a universal explanation of all syllogisms, instead of applying only to some of them.

(2) A far more effective motive for this particular explanation is to be found, not in Mill's theory as to the origin of knowledge, but in his view of the attitude of the logician towards phenomena. We have indicated in a former chapter some of the consequences of what may be called 'over-objectifying' the attitude of Logic. It would hardly be possible to find any better illustration of this tendency than is offered by some of Mill's remarks on the subject now before us. He maintains, for instance, that whoever asserts that 'All *M* is *Q*' must know, or, if not, ought to know, that '*P* is *Q*', because the fact that *P* is *Q* *is* simply a part of the general fact that 'All *M* is *Q*'. "The proposition, Socrates is mortal, is presupposed in the more general assumption, All men are mortal: we cannot be assured of the mortality of all men, unless we are already certain of the mortality of every individual man", including of course Socrates. Such a statement as this surely involves some want of appreciation of the distinction between the objective facts and our subjective recognition of them. It is quite true that when we contemplate the facts without any relation to our processes of acquiring them; when we think of them as they exist in nature and not as they come into a syllogism; we see that the fact that '*P* is *Q*' is neither more nor less than a part of the broader fact that '*M* is *Q*'. But there is a very great difference,—for the purposes of Logic, all the difference required,—between saying this and admitting that our recognition of the former fact is necessarily and

simultaneously implied in our acceptance of the latter proposition.

(3) This brings us to the only point which seems to involve any serious difficulty. Is it possible, then, when we thus shift our standing from this ultra-objective position, to recognize the existence of a distinction between the premises and the conclusion which when viewed from that position did not appear to be a distinction at all? Put into plain words, can we suppose a mind which has accepted the two premises and which has *not* yet accepted the conclusion? If there be any real step of reasoning involved in the process, then we ought to be able to make such a supposition as this; for, however short and rapid that step may be, we can yet imagine ourselves, if we are only quick enough, catching the mind just at the moment before taking the step. And if we could, so to say, take an instantaneous photograph of the operating mind, we should find it in the possession of premises but not in possession of the conclusion. It is the supposed impossibility of realizing such a state of things as this that has always constituted, I apprehend, the principal difficulty in the way of accepting the Syllogism. Thus Dugald Stewart puts it very plainly: "Is it possible to conceive an understanding so framed as to perceive the truth of the major and minor propositions and yet not to perceive the force of the conclusion? The contrary must appear evident to every person who knows what a syllogism is" (*Works,* Ed. Hamilton, III. 74). And the reader of Mill's *Examination of Sir W. Hamilton* will remember how he works the same objection when criticizing the general Conceptualist doctrine of Judgments as expounded by Hamilton.

This criticism does not seem to me to be at all convincing. It derives nearly all its apparent force from the extreme simplicity and familiarity of the examples commonly selected, combined (as above remarked) with a want of retention of the distinction between the phenomena regarded as a part of nature outside us, and the selections and appropriations of these phenomena which enter into our judgments.

The question before us, it must be clearly understood, is narrowed down to this. Is it possible for any one who has consciously realized that M is Q, and P is M, *not* to consciously realize that P is Q? If so, then, however rapid and however

brief the step may be by which he travels from one point to the other, a succession of such steps will lead him a long way: in fact, as far as we want him to go. Look, for instance, at the nature of this step where it presents itself concretely and intuitively, as in a geometric diagram; say, in one of the Eulerian illustrations of the syllogistic figures. It must be admitted that if we draw three small circles on a blank sheet of paper, it is impossible to conceive ourselves as occupying the critical point in question, for the eye will take in all the three figures simultaneously. But suppose that there are a multitude of figures drawn all over the paper, and that these are of many and very various outlines, and the problem assumes a different aspect. It is then quite possible for any one to have traced out the inclusion of M in Q, and subsequently that of P in M, and then by a distinct mental act to put these statements together so as to realize the inclusion of P in Q.

What thus applies to concrete figures applies also, it may be urged, to propositions generally, when these are interpreted in the sense explained in a former chapter. When, on the other hand, we adopt the Conceptualist interpretation it certainly is less easy to support the same contention. This is the point so vigorously worked by Mill in his polemic against Hamilton. In this case M, P, Q, stand for concepts or bundles of attributes. The admitted simplicity of these elements,—for they must be considered as the results of abstraction, and as having been therefore stripped bare of everything but the few attributes which constitute them,—makes it almost impossible to put ourselves into the desired position of partial appreciation of the mutual relation between the three. The case here is much the same as in the artificially simplified example above of the three circles drawn on clean paper. I cannot indeed agree with Mill that it is quite impossible for any one to have realized that the concept M is a part of Q, and P a part of M, without simultaneously realizing that P is a part of Q. But the difficulty of putting ourselves into such an instantaneous position must be admitted. What Mill's polemic seems to be really available against is the Conceptualist theory of judgments, of which this state of things is an outcome, rather than the foundation of the syllogism when this is expressed in a less subjective form.

That the man who has realized in any concrete form,—for the symbolic statement intensifies the apparent difficulty,—that All M is Q and that P is an M, and who does not know yet that P is a Q, is standing, so to say, on an extremely narrow ledge, must be admitted; but the only question is, whether it can afford him a possible foothold. If it can, as I have been maintaining to be possible, then he can make use of his momentary position to climb higher. And for a justification of the syllogistic process this is all that is needed.

An important consideration must be borne in mind here. In the Syllogism the processes of reasoning with which we are mostly concerned are extremely short. There is a great deal of labour and insight required perhaps for the acquirement of our major premise in the form in which we can employ it, and then nothing more may be demanded than a single almost instantaneous step of inappreciably small advance. If however our reasoning processes were carried on with the continuity and intricacy displayed in mathematics we should soon have obvious proof over what a distance we may have advanced by a succession of such apparently insignificant steps. Every one who has studied mathematics must have experienced a feeling of surprise at times in finding how far he has been unconsciously carried on in this way. He starts with a premise which it may take some trouble to distinguish from a pure identity, and finds that, starting from this, he may be imperceptibly led on by intuitively obvious advances into some profound and far-reaching algebraical formula. In the ordinary syllogism there is of course nothing corresponding to this, but if we were to select examples from the more complicated varieties offered under the Symbolic treatment of Logic it would not be difficult to find instances which should approximate to those in mathematics.

The view here adopted of the nature of the Syllogistic process and of its relation to Induction may therefore be summarily stated as follows. The common traditional account of that process seems a perfectly tenable one; that is, the objection against it that it involves necessarily a petitio principii, and only states as a conclusion what we must have known as a premise, is not valid. On the contrary, it is quite possible to suppose any one setting out from the two premises

as his real starting point, and reaching a conclusion which, regarded as a proposition or judgment held by him, may be something distinctly new. If this be so, it is not necessary, with Mill, to insist upon going behind those premises in order to enquire into the grounds of their original acceptance: the customary logical process of taking them for granted, and starting from them as the origin of our reasoning, is quite consistent and tenable.

But though it is permissible thus to start from the premises, and ask no question about how they were obtained, we are not necessarily bound to do this. When we do proceed to enquire into their warrant the answer becomes less simple, and may take one of two forms. Sometimes we should find that the original data on which the major premise was based are so close to hand that a moment's reflection will suffice to revive them. If the reader will recall an example which we have already discussed, (v. chap. V. p. 126) viz. that of a person being pronounced certain to die because he has been bitten by a cobra, it will probably be admitted that if the question were proposed to a number of persons who had no notion of illustrating or confuting any particular theory of the syllogism, we might get just as many answers suggestive of the deductive as of the inductive form of inference. If so, then those who adopted the deductive form, by assigning the ground 'All those who are so bitten die', would,—if there was the slightest delay in accepting this ground, or if anything directed attention to the reasoning, —at once admit that the individual observations were the real grounds of their assertion. Whenever this is the case; whenever, if one may so put it, the individual facts are so close beneath the surface that we may see them through the thin medium of the universal proposition, then Mill's account of the syllogistic process is the simplest and best. In these cases the deductive form is comparatively circuitous and artificial; the actual inference is to all intents and purposes inductive.

Often however the facts are far too remote to be thus readily reached. We hold, and hold with confidence, multitudes of propositions, for which we should find it impossible at the moment to assign facts in proof. It is quite consistent to claim that no general proposition can be a true *ultimate* starting point, and yet to admit that such propositions are

often the only starting point from which the thinker actually did set out, and often indeed the only ones from which he possibly could have set out. What in fact is commoner than to be convinced of the truth of some general statement, and to be convinced that we are justified in holding it, without our having the slightest recollection at the time what the data were which had at one time or another sufficed to convince us? Now a theory of the Syllogism which requires for its explanation and justification that the full account of it should straggle, so to say, over our whole life, if we are to find scope for both its premises and conclusion, is surely unfitted for the purpose of Logic. Nothing but a supposed insuperable difficulty, at some point or other, in the common explanation, could force us to any account so far-fetched as this; and that there is no such insuperable difficulty we have endeavoured to show.

Mill's explanation, indeed, if we insist upon extending it to such cases as these, seems really to be a transgression into the province of Psychology: an attempt to determine the *ultimate* sources of knowledge. It is very likely true that if we probed the sources of our knowledge to the bottom we should find that it all originated from particular facts; provided always that we were at liberty to appeal, if occasion required, to ancestral knowledge; that is, so far as we are concerned, to inherited acquirements. But Logic, as here conceived, is not a historical or psychological science. It must be prepared to start at any moment from our present standing point, or at most from a step or two behind this, and to offer an account of the connection of our beliefs over this limited range.

It seems therefore to follow that if we are determined to make everything give way to unity and consistency by admitting only one explanation of the syllogism and of its relation to Induction, we might accept either the common account or that of Mill; though, as a sole explanation, the former seems decidedly preferable. But it appears to me that we shall keep much closer in accordance with the actual processes of thought if we admit them both, each holding good in its own particular circumstances. Mill's account seems the nearest to the mark in those cases,—comparatively exceptional as they doubtless are,—in which we use the syllogistic form, or

an abbreviation which is naturally expanded into that form, but in which the basement of individual facts is so near below the surface that the shortest scrutiny will serve to detect them. And the common account, on the other hand, is the most satisfactory in that wide variety of cases in which we hold a proposition, though we cannot conveniently, and perhaps cannot at all, recall the grounds on which we do thus hold it.

CHAPTER XVI.

ANALYSIS; SYNTHESIS; HYPOTHESIS.

WE have had from time to time, in the course of the preceding chapters, to take notice of the general processes of Analysis and Synthesis. This reference was unavoidable, for not only do these processes meet us in the guise of deliberate methods of procedure,—as when we discussed their supposed equivalence to Induction and Deduction respectively,—but they also pervade every elementary operation which enters into Logic. We could not formulate propositions, we could not even conceive notions or employ terms, unless our own minds and the minds of others before us had been long and actively at work in the way both of Analysis and of Synthesis.

I. We cannot however get far in our discussion of these processes without having a third process suggested to us, which, for want of a better term, may reasonably be termed *Hypothesis*. It is by no means the same as those two, but it is inextricably involved in all their higher applications, and it seems to be time now to devote a chapter specially to its consideration. I am fully aware that the employment of the word Hypothesis, in such a comprehensive sense as is here suggested, may be misleading, since very possibly the principal associations with which it will be connected in the reader's mind are derived from the frequent caution inculcated, on the supposed authority of Newton, against its free introduction into Physical enquiry. But in default of any better term this will serve our purpose, and the best efforts will be made to prevent any misconception as to the sense in which it is here used.

The simplest way of introducing the subject will be by recurring to a distinction already pointed out when we were speaking about Analysis and Synthesis in a former chapter.

What we sometimes mean by these terms is the actual physical process of combining and separating the various elements involved: sometimes, on the other hand, we merely mean to refer to an exercise of abstraction, the *thinking* of the elements separately. The former is possible in the case of Chemistry, and therefore the usage of the term in this science is perfectly literal. In order to procure oxygen or hydrogen separately, we are not obliged to confine ourselves to 'thinking away' the other ingredients with which they may happen to be compounded; we can remove them in their concrete reality. The separate elements are therefore of the same order of concreteness as was the compound from which they were obtained; and if we want to recombine them, say in order to produce water, we have only to carry out the proper means of doing this. In other cases, —as generally holds where organic phenomena are concerned,— we are often able to effect the analysis in this sense, but are not able to carry out the converse synthesis.

Now compare with this the case of Mechanics. When, for instance, we are said to 'resolve' the motion of some moving body, are we dealing with as concrete realities as the chemist does? A body is moving, say, along the diagonal of a parallelogram, and we say that this motion may be resolved into two others corresponding respectively to two adjacent sides of that figure. We are not supposed to be touching the moving body, and therefore it may seem as if we were only dealing with the phenomena through the agency of the mind. The actual motion however might have been produced by a combination of two others, that is, by a true physical synthesis. Suppose, for instance, we carry a tray in a direction parallel to one of its sides, whilst a ball is set rolling on it in a direction parallel to an adjacent side; the real motion of the ball relative to the room (provided the two component motions were properly adjusted) will be in the direction of the diagonal. In this case, if the actual path traced out by the ball were all that is shown to any one, he could not know that it had not been produced by a body moving under one single impulse. Among such phenomena as these, therefore, we seem to be in an intermediate position between what may be called physical and mental Analysis and Synthesis.

Once more. When we are dealing with individual events

of a very comprehensive and intricate kind, whether these be physical or moral,—say the production of the existent order of nature on the earth's surface as discussed by the geologist; the outcome of a long play of motives as displayed in the character of some one whom we know; or the whole succession of mental and moral phenomena which go to make up what we call a political revolution,—what is the nature of Analysis here? It is obvious that such performances as these cannot be repeated at will; still less can we display separately the elements which constitute them. Accordingly the process of Analysis must be understood in a very different sense from that in which the chemist regards it.

II. The mere suggestion of such examples as those above seems to remind us that, along with the processes of Analysis and Synthesis, we must take account of another, which is quite as important and as far reaching as either of these. Directly we proceed to apply the former to any but such phenomena as are well within our practical grasp, we find ourselves confronted by the question, Under what circumstances and to what extent are we at liberty to imagine things otherwise than they actually are? Remember that what we are here speaking of is not mere *Abstraction*: that is, we are not merely thinking of this or that aspect of one of these complicated series of events. What we are doing, in this Analysis, is to conceive elements existing separately which we know never do so exist. This chapter will be devoted to a detailed examination of the nature and the warrant of the process here referred to.

To the best of my knowledge and belief such an enquiry as this has never been made. The extreme familiarity, and the apparently undisputed license, of freely making suppositions in every direction, and over whatever range we like, has probably led most of those who write on our mental operations to take it for granted that there was nothing here to call for serious discussion. This, we think it will be found, is very far from being the case; but we have to face a consequence of this neglect in the total absence of any well recognized name for the operation in question.

The word which will here be used, for want of any better, is Hypothesis or Supposition. To understand the sense in

which this word is employed we need only refer back to that assumption of a fundamental Duality which has been already insisted on as demanded for a Material or Objective Logic, and indeed for any rational discourse about the world of phenomena. Recall for a moment the contrast between the course of nature, or great complex of objective facts which exists without us, and our own mental pictures or representations of this. The two ought, as a general rule, to be in harmony with each other. And so long as this harmony prevails we have a variety of names for our various mental pictures of the external events, the distinctions as thus named turning mostly on the different time relations between the two. If the conception be supposed to have its corresponding objective counterpart in the future, then we commonly call it an anticipation, forecast, prophecy, presentiment, or so forth, according to the nature of its warrant and the source from which it is derived. If it refers to the past, and is within the sphere of our own personal experience, we call it a recollection: if it was derived from the experience of others we should generally consider it as embodied in a narrative, a testimony or a history. If it refers to the present it is not so familiarly recognized and named, though the same essential relation between the two elements exists, and these may be similarly compared. For instance, my mental picture of a cricket match, which is now going on in some other part of the country, stands in a certain relation to that event itself; it must be right or wrong, or partially right and wrong, compared with what we may call its original. But the relation here is generally such a very transient one that we have not got special names corresponding to 'anticipations' or 'recollections'.

In the foregoing cases the conception was supposed to be a correct one. At any rate if it were not so, it was intended to be so, and any lapse from correctness was a defect. Now what is here understood by a Hypothesis is any such concept or mental picture when it is either known or suspected *not* to be in accordance with facts, or at best put forward tentatively as what may eventually be found in such accordance. It is not a merely mistaken one, held for true and afterwards found to be wrong, but is deliberately entertained in the doubtful attitude, and shows its essential character whilst it is so

entertained. Like the correct conceptions, treated above, these also admit of subdivision into various different kinds, but the distinctions in this latter case will be found to turn on another principle. These Hypotheses are not popularly distinguished according as they refer to events in the past or the future,—the reasons for this we shall see more clearly presently,—but rather according to the purpose they are intended to fulfil, the seriousness with which they are entertained, or the elaborateness of their construction. When they are the serious or semi-serious suggestion of a scientific man, adapted for purposes of physical illustration, they may be dignified by the name of Hypotheses. When they are conceptions referring to some trifling event, especially when they are taken up on very light grounds, they mostly go by the name of Guesses. When they are the elaborate constructions of a poet or a novelist, dealing perhaps with a whole group of events in a long narrative, they go by such names as that of a tale or a poem.

The main facts thus suggested are so familiar to everybody that one really feels as if some apology were needed for demanding that such a well-known process shall be accounted for and justified. And yet when we come to view the whole question scientifically it does really look as if there were an opening for some serious explanation. The general aim of all rational observation and speculation is so obviously that of securing a complete and accurate correspondence between the mental and the physical, between what we think and conceive within us and what we observe and feel without us, that we may well enquire,—even at the risk of being thought for the moment to ask a foolish question,—what exactly is the justification of frequent and deliberate departure from this general aim. How does it come to pass that we often find it so helpful to conceive the course of events to be other than they actually are, when our final aim is only to conceive them *as* they are? And granting the utility of these conscious divergences from what we may call 'the truth of things', we ought still to ask if they are subject to no kind of control or limitation? If we may 'suppose' some things, why not anything whatever?

My own opinion is that the whole exercise of this important and far reaching procedure, by which, in its broader

flights, we contemplate the wildest and most impossible physical combinations, has sprung from very small beginnings. This habit of Hypothesis has its roots in the necessities of daily and primitive life, having commenced in fact amidst the inevitable mistakes and shortcomings of human faculties. But having shown its usefulness and indispensableness there; having engrained itself in our habits of thought and having fitted itself with a peculiar and appropriate form of speech; it has adapted itself to continually wider requirements. The result is that we not only find it highly convenient in the exigencies of daily life, but that we have also converted it into one of our most powerful scientific instruments and into one of our most effective emotional and illustrative aids in the general pursuit of Science.

The process may be tracked back to that very familiar state of things in our practical life in which we are in doubt between two or more alternatives. Without the slightest wish deliberately to contemplate the false or the doubtful;—on the contrary, with the most pressing inducements to entertain only what is true;—we yet find that daily life cannot go on without contact with the former. However true it may be that the ideal state of knowledge is that in which we can at will summon up with certainty any portion of the whole course of events around us,—past, future or present,—we, in our actual condition, are still indefinitely remote from this. Much of the range of practical, as well as of speculative life, presents itself to us in the form of *alternative* certainty, if of certainty at all. We may succeed in limiting the possibilities to two or three, and to making sure what we shall do ourselves or what others will do, according as one or other of these shall come to pass. Reasons were given, in Chap. X, in favour of the view that this state of things is the essential condition of, and offers the original field for, the use of the particle '*if*': in other words for the use of the hypothetical proposition.

This, of course, is carrying things a very long way back. We are thus referred to a primitive or practical use of the process, as evidenced by the occurrence of the hypothetical form of proposition in all civilized languages. But, starting from this narrow practical application, the same form has developed and adapted itself to far wider uses of science and of fancy. From mere hypothetical propositions and their originating

circumstances, we are led on to enquire into the nature and use of Hypothesis generally.

(1) The first of these developments which we will now consider seems really to be nothing more than a slight modification and extension of our attitude under very familiar practical circumstances. Take one of the simplest of cases, such as those which were referred to when we were discussing hypothetical propositions. We are, say, contemplating the weather of to-morrow. Will it rain or not? We cannot be sure: but we can form the two postulates of rain and fine weather, and we can say, 'If it rains, I shall go to the exhibition', 'If it is fine, I shall go to Richmond'. Here it is obvious that we are keeping as close as we can to the reality, that is, to the real future experience before us. We have not the slightest wish that our conceptions should vary from the facts. Our outlook is a most limited and practical one.

But having once become familiar with this attitude and having acquired a peculiar form of speech in which to embody it, we soon find that the same form will help us in other cases. This distinct contemplation of alternatives in a doubtful frame of mind, as indicated above, is primarily confined to the future and was not intended to simplify a difficult problem. But we soon come to find that it can be applied to the past as readily as to the future, and that it may become a powerful aid in the way of simplification and discovery. For instance, a man has been murdered, and we are endeavouring to discover who it was who perpetrated the crime. The hypothetical attitude and form of speech come in here most usefully. Though originally intended for prospective purposes, we soon see that it will readily adapt itself to occasions which are retrospective. I begin to think over the various possible agents. I say to myself, 'If it were *A* who did it he must have been away from his work at the time: but he is known on the contrary to have been in the shop all day'.—'If it were *B* he must have got possession of a gun: but how, and what has he done with it since?'—'If it were *C* he would have been suspected at once, and his appearance on the scene would have caused alarm'.—'If however it were *D* there seems no such difficulty to be accounted for'. And in this way we may proceed successively through a string of such suppositions.

This, of course, when we take account of the whole procedure from the assumed starting-point to the conclusion, is what was described in a former chapter, when we were engaged in discussing the geometrical usage of the term, as the Analytic Method. At present we are mainly concerned with that starting-point regarded as a supposition or Hypothesis. This use of Hypotheses, in which a supposition is made in order to account for a given fact, and which we may term the *constructive* use, seems to be nothing more than a development of the practical use of common life; and had not the wants of this provided us with a handy but peculiar form of speech for the purpose, it is hardly possible that we should ever have found ourselves able to appeal to such a resource with the success we actually attain. There can of course be no question that we find the greatest assistance in thus setting up one supposition after another when we are engaged in unravelling an intricate problem. Our faculties being as limited as they are, and the processes of nature and of human conduct being as complicated as they are, we can seldom see far before us or behind us. Even in the most abstract of sciences, mathematics, we cannot always succeed in thus unravelling more than a short skein at a time.

What we do therefore is to invent possible cases one after another and trace their consequences. This is peculiarly a method of enquiry or investigation, as distinguished from one of exposition. Every enquirer, for instance, would naturally adopt some such resource if he had to account for an event such as the murder referred to above, where it did not appear obvious at first upon whom suspicion should fall. And what we thus resort to in the affairs of common life the scientific observer will also find serviceable in his own department. He contemplates one possibility after another, using each of these merely as a sort of scaffolding which may be useful in the erection of a more solid structure, but which would be swept away as soon as it had served its purpose.

So much then for this 'constructive' use of Hypotheses. All the advance which we have so far contemplated consisted in transferring our suppositions from the practical wants of the immediate future, and employing them to account for the past and the present: using them, that is, for a purpose which is either scientific, or at least of speculative rather than of merely

practical interest. The conceptions which we thus entertain, however, though often doubtful, were never considered to be certainly false.

(2) But the extension by which this point has been reached admits of being carried out further. Just as we found that there was no necessity to confine our suppositions to the narrow limits represented by our own possible future experience at any particular conjuncture, but that they would play a useful part when applied to the past or to the experience of other persons, so we may come to perceive that there is no necessity to confine them to the limits of any possible experience at all. What are we to say, for instance, to the various hypotheses which meet us on the pages of any work on mathematical Physics or on Political Economy? We may there find the writers postulating not merely special facts, but also general laws, such as we know for certain never did happen and never will. In the course of some discussion about the rotation of the earth around its axis we may find ourselves bidden to conceive the velocity suddenly increased, and asked to determine the consequences in reference, say, to the water of the sea: it may be proposed to determine what must be the increase of velocity to drive all the water off the surface of the earth. So in some works on Political Economy, in the course of a discussion about the currency, we may have the question proposed as to what would happen were the amount of coin in circulation to be suddenly doubled or halved. Or, in the course of a discussion upon Capital, it may be asked, What would happen were all productive labourers to cease working for a certain time? Every reader of the Science knows how frequent are these imaginative suggestions.

Now what is the meaning of all this? Such suppositions as these are not simply borrowed from Wonderland: they are not meant merely to startle and amuse. They are proposed by the soberest and most exact writers quite as often as by any others, and are therefore certainly designed for some scientific purpose. That purpose, doubtless, is *illustrative.* The practical and constructive aim is not of course entirely absent even here, but it is very remote, and is quite subordinate to the speculative. Such employment may be compared to a sort of fencing exercise which we take, not for its own sake but to make ourselves thoroughly familiar with the use of our weapons for

more serious purposes. These hypotheses,—or problems, as they are often called when deliberately proposed for solution,—may be very useful in familiarizing us with every possible combination of events. In order to attain a clear comprehension of the bearing of a law in the complicated combinations in which it may present itself in nature, we may find it necessary to begin by working out the results which would follow from it and from similar laws, in a number of simpler but actually non-existent examples.

III. Inasmuch as no two combinations of circumstances, in subjects of a complicated character, ever will be found to agree in all respects, the reader will see that we are here being brought round to the topics of Analysis and Synthesis. But before reopening any discussion about the nature of these processes, it will be well to enquire what are the limits of admissibility of Hypotheses such as those just indicated? Under what conditions, or over what range, are we entitled to resort to them?

(1) The first of these conditions is of a somewhat formal character; that is, it limits the nature rather than the application of the hypothesis. We may express this condition briefly by saying that the conceived change in the natural career of events which is mentally introduced when we make a hypothesis, and which in fact constitutes the hypothesis, must be perfectly determinate. It can be made so because it is arbitrary, since any supposed change which we introduce ourselves is in our own power to make it what we will. The framer of the hypothesis ought to be able to assign precisely the nature and the limits of the change which he mentally contemplates; and he should recognize that everything which he does not thus change, or which is not implicated in what he does thus change, remains unaltered; that is, is left to develop itself according to its own natural laws. Our hypotheses must always presuppose that there is an established order of nature,—for speculation of a rational kind could not take a single step in true chaos,—into which we suppose ourselves introducing some perfectly definite change. This change may be of various kinds; but it will generally consist either in some alteration of the general laws which govern the course of events, or in some single specific alteration in these events themselves. It may be, for instance,

that we are supposing that the law of attraction of gravitation is altered from that of the inverse square of the distance to that of some other power. Or it may be that we are supposing a single change, once for all, in the velocity of rotation of the earth, that body being then left to obey the recognized laws of motion. Clearly in any such cases as these we can state precisely what it is that we have supposed to undergo alteration and to what precise amount; and assuming that the general laws, and particular collocations, remain unaltered in all other respects, we trace the consequences of the hypothetical innovation. In Physical Science, at any rate, the slightest haziness as to the limits of our hypothesis would never be tolerated. And we may say the same in the scientific treatment of Political Economy. Every student of that science knows how suppositions of the wildest character,—if regarded from a practical point of view,—are frequently made by sober writers. Such authors postulate changes of conduct, or of physical elements, the real occurrence of which in any community would indicate either that the society was composed largely of maniacs or that it was likely soon to assume that character. But these writers know exactly what they mean, and they will be found to impose the strictest limits upon the range of the contemplated alteration. If they postulate 'that all capitalists cease from saving during one year', they assume all other motives and courses of conduct to remain unchanged, except in so far as derivative changes are implied as results of the one great change which they contemplate. And so in any other science in which what we are here discussing as 'illustrative' hypotheses are introduced. Hardly any restrictions need be imposed upon the magnitude and range of the initial contemplated change, provided only that its limits be strictly defined and that these limits be consistently adhered to.

We shall best see the significance of these very obvious requirements by examining a few examples in which they are infringed. We have not far to seek in order to find glaring instances in point. Almost any hypothesis within the province of Theology, or of Morality treated from a theological point of view, will serve our purpose. For instance, the supposition has been occasionally put in works on the Evidences of Christianity, or on Systematic Morality,—'What would it be our duty to do

were an immoral command conveyed to us from the Deity?' or 'What effect upon our belief should be produced by a miracle worked for some clearly absurd purpose?' Let us pause for a moment to consider what is really involved in such suppositions as these. Any rational conception of the Deity, as indeed of an Ideal of any kind, must always imply, and be grounded upon, a very complicated synthesis of intuitions, emotions, and of inductions founded on these. Hence to conceive a serious alteration in any important group of these constituents would by implication demand a reconsideration of the whole synthesis. What was supposed to be removed or changed would react on what was left, as it was thought, unchanged; would disturb its balance, perhaps break up its cohesion, and thence bring about a profound alteration of the ideal itself. No doubt to a polytheist questions affecting the character of the deities would be quite legitimate. If there are plenty of gods, mostly little better than men in their moral character, no man could undertake to say what sort of commands might not be expected to issue from them. But surely to a believer in a Perfect Being such a supposition is idle. In proceeding to frame it, we are no longer postulating a finite and determinate alteration, leaving all outside its limits unchanged, but we are postulating one which breaks into and disturbs the very starting-point from which we are supposed to set out. If by any possibility it could be clearly proved to me that a miracle had been worked for some wicked or ridiculous purpose, I could not simply start from this point and reason onwards. I should want to "try back" a long way and reconsider the whole evidence which may be urged in support of the existence of a wise and perfect ruler of the world.

The fact is that suppositions of this kind verge towards, even if they do not often become, a contradiction in terms. Thus I cannot but think that the mere raising of the question, what should be done on the occasion of an immoral command from the Deity, stands upon the same level with one which should ask what would become of such and such properties of an ellipse if it should happen that the foci were not at the same distance from the centre? The mathematician would of course reject the question if couched in those terms. The utmost he would admit would be that a curve somewhat resembling an

ellipse might have two points corresponding in some respects to the foci, and yet at unequal distances from the so-called centre. But before even discussing the subject he would require a clear definition of what was meant, in other words a restatement of the problem in perfectly accurate terms.

The suppositions sometimes put as to the consequences of a general alteration of some fundamental conviction on the part of mankind seem to me mostly to suffer from the same fatal vagueness. Thus it may be asked—as in fact it often is asked,—what would be the effect, upon the conduct of men and the institutions of society, of a general abandonment of the doctrine of immortality? If this question be proposed positively and anticipatively, that is, if it be asked what *will* be the effect when such belief has disappeared,—proposed, that is, by some one who has a definite theory as to the correlation of our beliefs and the order in which they will succeed each other,—the question is reasonable enough. But then such a person is not merely making a supposition: he is sketching out future history; and he is understood to be describing all the principal modifications of creed and conduct which would necessarily accompany the one change specifically alluded to. But when any one proposes the question in the form of a hypothesis we must insist on his defining its limits. Let him say exactly what he supposes left untouched behind,—as the physicist is always prepared to do,—whether and to what extent the customary estimate of the value of life, of the scale of our hopes and fears, the nature of domestic and public education, and so forth, are retained; and if not to what extent he supposes them altered. Then only would the problem be stated in terms which the physicist would tolerate if any similar supposition were introduced into his domain; and the conditions are just as much demanded in one department as in the other.

(2) Another requirement which we ought to insist upon is that the group of events into which we introduce our hypothesis shall not be *unique*. Some of the examples mentioned above seem to me to err in this respect also, as well as in being ill-defined in their limits; but it will be well to discuss this ground of objection separately. History,—that is, narrative history rather than what is termed the philosophy of history,—seems to offer the typical instance of this class; for here we

have a sequence of events which from the extent of ground they cover and the slow and continuous process of evolution to which they are subject, do not offer a repetition of parallel cases. That is, each of them is, to the degree of minuteness of identification to which we find it desirable to proceed, as near being unique at every point as anything can be.

Suppose then that any one is engaged in describing to us the course of past events in such and such a time and country. Is there any justification, or indeed any significance, in his breaking off at some point, and proceeding to describe what would have happened *if* something different had occurred there from what did actually take place? Can he explain to us what he means by making a digression in this way from the actual to the confessedly imaginary? Doubtless the mere narrator does not often indulge his fancy in this way. Still we do occasionally find interpositions of the following kind, made in all sobriety by really earnest students of history:—"No one can doubt that the Roman republic would have subsided into a military despotism if Julius Cæsar had never lived: but is it at all clear that in that case Gaul would ever have formed a province of the empire?" Mill, again, in his Essay on Liberty, has offered the following reflections:—"It is a bitter thought how different a thing the Christianity of the world might have been if the Christian faith had been adopted as the religion of the empire under the auspices of Marcus Aurelius instead of those of Constantine."

In order to see what sort of purpose is served by such speculations as these, and whether or not they lie within the limit of what is rationally permissible, let us take a few more examples. Try some which it shall be universally admitted lie respectively within and without that limit. Presumably no one has read the account of a battle, for instance, without the supposition inevitably rising in his mind, whether or not the historian has gone into the discussion of it, What would have happened *if* so and so had been otherwise? If Nelson's fleet had not returned in time from the West Indies before the battle of Trafalgar, and if Sir R. Calder's action had not been fought: would the contemplated invasion of Britain have actually taken place? Had Grouchy been a little more prompt in his movements, or had a British square given way, what would have

been the result at Waterloo? Every one falls so instinctively into such conjectures that we feel somehow that they must be not merely permissible but serviceable. Now contrast with the foregoing such as these, which we might consider to be made by some ingenious theologian:—Had Adam never fallen into sin, would man have remained perfect? Had Judas not betrayed his Master, would the Crucifixion have taken place? Had St Paul never been converted, the work being left to the other apostles, what would have been the difference in respect of the evolution of doctrinal Christianity?

It must not be urged, as against these latter examples, that the events could not have happened, whilst the former could. No contradiction in terms is involved in their proposal, for they only suppose man to have acted differently from what he is known to have done, but not in any way to have contradicted his nature. Of course, when we look back upon the past, anything that has once happened cannot possibly be changed,—such a champion of Divine Power as Bishop Pearson admits the truth of the old saying on this point,—but this is not what we are supposed to be contemplating. When we are discussing a conceived or postulated change, the only valid objection of the above kind would be one which should establish a difference between one such class of cases and another. Unless therefore we introduce the Fatalistic doctrine in a rather coarse theological sense by maintaining that St Paul, Judas, and Adam respectively were predestined to act as they did, whereas Nelson, Calder, and Grouchy were free in their actions, no difference of a relevant kind can be detected. If by 'possible' we merely mean that, given the requisite volition on the part of the agents at the time, the effects contemplated would follow; then both classes of events are alike possible. But if by 'impossible' we mean that that volition did not as a fact exist, then both alike could not possibly have failed. We must therefore seek for some other ground of distinction, between admissible and inadmissible hypotheses in this department of events, than one which would find it in the assumption that one kind supposes what is possible and the other what is impossible.

The real ground of distinction here seems to be of the following nature. Whenever an event or course of events is unique,—or in so far as it is unique,—it does not appear that

anything can be gained by supposing it to have been different from what it actually was. For instance, the theologian who is investigating the history of the church or of the world, in so far as he conceives it to display the divine will;—or the secular historian who is doing the same sort of thing on some evolutionary theory;—has simply no object, of a rational kind for inserting an '*if*' at some stage, and linking on at this point an alternative career different from that which he knows the actual career to have been. It is where we are dealing with a course of events in which the actions, the persons, the motives resemble those of the present day that we find it serviceable to postulate imaginary variations. This must generally be the case where we are dealing with ordinary events in some degree of detail. Doubtless any particular battle, say, taken as a whole with all its political surroundings, is unique. But it may be separated mentally into parts,—that is, we may detect features in it,—which recur from time to time, and which may therefore offer close comparison with what we may again experience. In so far as this is the case any such hypotheses fall into the class of those which we have termed *illustrative*, of which we find such numerous instances in works on Physics, especially on mechanics, and also in such a subject as Political Economy. When, for instance, we postulate a change in the velocity of the earth's rotation, the change in itself, regarded as belonging to a unique course of physical events, is as mere a flight of imagination as any which should suppose some corresponding innovation in the historical course of human belief. Similarly when we postulate, say, some sudden change in the currency of England, or a complete cessation from work on the part of half the labourers of the country. But then what we have in view in all these assumed cases is the existence of parallel instances. There are other planets or suns besides our own, and these we know to be in very various stages of evolution; and, if we like to widen the comparison, there are plenty of other bodies which may be made to rotate with any speed we please, and thus to furnish parallel instances.

Concluding then with a brief summary, we may sketch out the origin and functions of these processes of hypothesis as follows. The world that we want to know, that is, to systematize and infer, is infinitely complex in its extent and the variety of

its details. Accordingly every intelligent agent, at every point of his career, finds himself in some difficulty as to forecasting his conduct. He has, however, ages anterior to reaching anything resembling introspection or logical procedure, attained a standpoint of experience which from time to time limits the future immediately before him to two or more alternatives, each with tolerably certain consequences attendant upon it. This is the familiar position which has given such general prevalence to the hypothetical form of speech:—If A then B; if C then D; and so on. But having thus familiarized ourselves with the use of alternative suppositions, and having evolved a suitable grammatical form for them, we soon perceive that they are not confined in their application to the circumstances which originally gave birth to them, that is, to our own immediate future; but that they will serve to give frequent help in accounting for the past. We can project ourselves back into the position of agents to whom the facts to be reckoned with were future. We can say;—If there had been A then there would have been B: if there had been C then there would have been D; and so on.

The simplicity and familiarity of this step must not blind us to the fact that it represents a real advance and an exceedingly important one. Not only is it a very appreciable extension of the use of hypothesis, but it leads us on to the employment, in a formal and intentional way, of the processes of analysis and synthesis. These had of course been exercised to a great extent already, as evidenced by the whole fabric of language, but the process had been an almost unconscious one. But when we take to framing suppositions about the past in order to account for the past or present,—in a word when we begin to employ what may be called constructive hypotheses,—we generally have to exercise a good deal more of these processes of analysis and synthesis. The materials for the practical suppositions mostly lie ready to hand: they are presented to us from without rather than framed by us from within. But when we step back to a supposed past experience which is not presented to us, we have to originate for ourselves. A good deal has to be done in the way of putting things together to frame the antecedents. In a word, the hypothesis does not take the simple form of merely repeating a concrete element

which has been already set before us, but it constructs these elements anew for itself. There is only one way of doing this. We cannot create really new experience: the elements at least must have been already given: the novelty consists in selecting the materials from this and that group of prior experiences, and building up a new compound with them. It seems therefore that any extended use of Hypothesis for inferential purposes necessarily involves constant resort to the processes of Analysis and Synthesis.

But once started on the career of framing hypotheses; once having found that there is no necessity that our mental constructions should conform to accurate fact, we find it difficult to stop. There is a fascination which we all experience in simply evolving long trains of purely fanciful experience about ourselves or others. But as this serves absolutely no scientific purpose, nor even any purpose that can appropriately be discussed in a work on Logic, we need not enter into it here. Suffice it to say that the exercise of the fancy deliberately as such,—that is, the composition of consistent stories which are known not to be true,—is probably not a very early exercise of the human mind. To us at the present day it may seem of immemorial antiquity, but all that we know of really primitive man suggests that the clear distinction between the real and the fanciful which would thus be implied, demands some mental progress. We of course, at the present day, are familiar enough with the distinction. The final outcome of that mental faculty, which in the province of science we regard as Hypothesis, seems to me to be represented by that almost aimless license of fancy which goes by the name of building castles in the air. When the results are deliberately put together and are embodied in language, we have the fairy tale or pure romance. And so, if we cared to trace out the connexion, we might notice the intermediate links which, indulging in less and less departure from known fact, help to bridge over the gap which separates the novel from the history. But of course this does not belong to our present subject. Directly the exercise of the constructive fancy is devoted to the purpose of mere amusement, or to that of exciting or controlling the æsthetic or other emotions, it passes out of the range of the logician.

If the line of enquiry sketched out in this chapter be

accepted as sound it will serve, we may hope, to throw some new light on a topic which has given rise to considerable discussion in Logic; with a reference to which we may briefly conclude this part of the subject.

The topic in question is the legitimacy of 'Hypothesis' in the comparatively narrow sense in which it is commonly understood. Almost every writer on Induction or Systematic Logic has discussed the question whether Hypotheses are allowable at all in Science; and, if they are, how we are to distinguish between the legitimate and illegitimate kinds. When we take regard of the narrower sense in which these writers have employed this word, as compared with that in which I have ventured to use it in this chapter, the attempt to put limits upon the exercise of the faculty is more justifiable. One cannot but think, however, that if a clear distinction had been made as to the different purposes which our hypotheses can fulfil, the question would have been much simplified.

When the question is raised, What hypotheses are allowable in science? no simple answer can be given; the limits of admissibility must depend upon the particular purpose for which the hypothesis is introduced. It may be that no other aim was in view than that of employing them for purposes of illustration and explanation. When this is so, and when the class of objects to which they are applied is one which offers a plurality of analogous cases, it would seem that hardly any conceivable divergency from actuality which does not lie outside the limits indicated by such analogy can be objected to. We have no reason under these circumstances, to trouble ourselves about physical impossibility or absurdity. On the other hand, however, when the hypothesis belongs to the constructive class, the limits must clearly be much narrower, though it does not seem possible to assign them with any approach to success. It must be admitted, indeed, that such attempts have not often been made. Most of the recommendations in this direction have taken the form of good advice rather than of definite regulation. The cautions against rashness in framing hypotheses may fairly be classed with such a piece of sanitary advice as (say) the injunction not to over-tire oneself;—a suggestion which may be most valuable when coming from a good judge who is on the spot and who can appreciate the conditions in question,

but which does not help us much when we try to apply it for ourselves.

The presumption must of course always lie against the man who advances a really novel hypothesis,—whether this take the form of proposing a new law of action, or a vast extension of an old law, or even a new agent—but such presumption should not take the form of denouncing him for making a guess. The more active his fancy and the freer the scope he allows it, the better for him and for us, provided he does not trouble us and waste our time by, so to say, thinking and guessing aloud. All that is wanted is, not restriction of the hypotheses that are made, but only reticence as to those which are published or declared. There is no need to appeal to the History of Science, or to urge the practice of Kepler, on the one hand, or the over-quoted remark of Newton on the other hand. Everyone who has ever had to work out the solution of any little matter in daily life which had puzzled him, knows how many and how wild were the guesses that flitted through his mind before he paused at one which seemed more hopeful than the others. The larger the stock from which he has to draw, the better is his chance, other things being equal, of finding a good specimen amongst them. And the same holds good of the more serious speculations of the scientific man. We cannot therefore offer even such a piece of good advice as that he should not be too hasty in framing his hypotheses: we must confine the monition to their premature publication.

CHAPTER XVII.

INDUCTIVE METHODS.

SINCE the publication of Mill's treatise, every reader of Logic has become more or less familiar with the method of symbolic representation, by means of letters of the alphabet, of the various modes of solving the Inductive Logical problem; in other words, of symbolizing the well-known Four Methods. And this familiarity has been increased, not only by such works as Jevons's *Principles of Science,* but by a number of more popular handbooks. The result is that every student in an examination can now undertake to exhibit to us the exact process by which physical antecedents are eliminated, and the true 'cause' of any phenomenon is determined.

One is inclined sometimes to wonder what proportion of these students have any adequate conception of the real relation of such letter symbols to the phenomena which they represent, or of the conditions under which this representation can be admitted as suitable. Since it is extremely desirable to bring the difficulty clearly home to the students' mind we will depart from the usual course of exposition. Instead of attempting to plunge at once into the intricacies of what we may call 'wild nature',—an attempt which almost necessarily leads to our taking the example up at a point in which the main difficulties must be assumed to have been already surmounted,—we will commence with the careful study of a 'tame', or artificial example, of a kind which the symbolic procedure is really and thoroughly adequate to represent.

The requirements for its fitness are, briefly speaking, as follows. We must have some phenomenon before us regarded as a consequent or effect, which occurs repeatedly and whose cause

we have to determine. There must be a finite number of possible antecedents which can in different cases immediately precede it, these antecedents being distinct and separable, but capable of presenting themselves in a number of different combinations. Some one or other of these antecedents,—for we must admit, remember, the plurality of causation,—or of their combinations, is the 'cause' of the phenomenon: that is, wherever this occurs the phenomenon will follow. Our object is to determine all the possible alternative efficient elements of this description.

I. Now the following will I think be found on examination to be a perfectly suitable example for the commonly accepted methods of enquiry. (The *grounds* of this suitability will best be discussed when we compare it presently with any serious physical example: at present we will begin by merely analysing and explaining it.) Suppose a hotel with the usual old-fashioned bell arrangement. A man is in the office, where a number of these bells ring from the various bed-rooms. He takes for consideration a single one of these bells and proposes to determine the 'cause' of its ringing, in the sense of ascertaining which of the rooms is connected with it. As some preliminary limitation of the problem may always be presupposed, we will take it for granted that he knows that the continued occupation of a room will always result in a ring, and that he has means of knowing which rooms are occupied at any given time. Also we assume that the same room cannot ring more than one of the bells. More than this he is not supposed to know:—for instance, he must not assume that a single room will suffice to produce a ring; it may take a combination of several to do this. Moreover, since we admit plurality of causation[1], *several* rooms may ring the same bell.

Suppose then four rooms, A, B, C, D, and let X stand for the ringing of the selected bell. To save needless trouble, since no possible confusion can thence arise, the letters A, B, C, D, may also stand, in the discussion of the problem, for the *occupation* of these rooms respectively.

Now there are sixteen possible arrangements as to occupa-

[1] The reader must not confound the plurality of elements which may be required to constitute the single effective Cause, with the Plurality of Causes each of which, as a cause, is of course effective by itself.

tion, according as all four, or any three, or any two, or any one or none, of the rooms are occupied. All these combinations may enter into the servant's experience, and we must therefore consider them all in turn in order to ascertain what information they are capable of yielding.

(1) First consider the case of the four rooms, A, B, C, D being all occupied, and the bell being observed to ring. This simply tells the enquirer that the cause, or causes, lie somewhere among the four rooms in question. If he was supposed to know so much as this already, he learns nothing new by this experience.

(2) On four different nights three rooms are simultaneously occupied, namely A, B, C; A, B, D; A, C, D; and B, C, D. On each of these nights he finds that the bell rings. This batch of experiences, taken together, tells him nothing as to which antecedent is the cause; but it does let him know that there must be more than one cause. The first combination tells him that some one or more of the three A, B, C, can cause a ring; the second that some one or more of the three, A, B, D and so on.

We can see here exactly what is the use of multiplying the instances in this second batch of experiences. Each single instance opens out to us precisely the same kind of possibilities as when we took all the four rooms together, but they are severally of a more limited range. By taking a number of such instances together, we gain in two ways:—(1) Affirmatively that is, by considering the *presence* of antecedents, we extend the range over which we seek for the possible causes. The first referred us to A, B, C; the second to A, B, D; and so on. But, by so doing, we of course effect no more in the end than was effected once for all when, as in the first case, we were able to secure all the possibilities in a single grasp. All that we have done in the latter case is to sum up our separate experiences, each of which partially overlaps the others, and thus to obtain in the aggregate what was given before at once. Where we mainly gain is, (2) negatively; by considering what antecedents are *omitted.* We now know that any single one of the four alternative antecedents may be omitted without loss of the effect. We conclude therefore that there must be more than one cause, and we know the range within which the causes must

lie; but more than this we are not as yet in a position to conclude.

(3) Two rooms simultaneously occupied, namely the following six pairs in succession:—*AB, AC, AD, BC, BD, CD.* The bell rings in all the first five cases, but not in the sixth. Here, as before, we may consider what is secured respectively by the observation of the presence and the absence of the several antecedents. As in the preceding case, the sum-total of the presences of the antecedents simply gives in the aggregate what may be gained by securing all the possible causes at a single stroke. But the absences of the antecedents are now rather more complicated in their yield, for they must be considered both as they are followed by the presence and the absence of the *effect* also. As regards the former,—namely, the fact that each of five combinations which were partial only, as regards the number of rooms occupied, can nevertheless produce a ring,—all that we learn is that there must be more than one cause. But we do not know, any more than we did in the preceding case, what that cause may be. It is in the case of the sixth experience, where the cause and effect are both lacking, that light begins to dawn upon us. When *C* and *D*, and these two only, are occupied, the bell does not ring: therefore neither of these is the cause; therefore finally, by the considerations previously adduced, which showed the existence of more than one cause, we determine that *A* and *B*, and they only, are actual causes of the observed phenomenon.

(4) One room only occupied at a time, namely each of the four, *A, B, C, D*, on different nights. In the case of *A* and *B* the bell does ring, in that of *C* and *D* it does not. This case is, of course, simplicity itself. The two affirmative instances, in which a cause and the effect were both present, show us two antecedents that *are* causes: the negative instances, in which the cause and the effect are both lacking, show us that the two other antecedents are *not* causes. Our knowledge therefore is complete, so far as methods of this sort can carry us.

In the foregoing examination we have noticed every case which can possibly occur under the conditions assumed, and therefore we have before us all the Methods of non-quantitative investigation,—whether that of Agreement, of Difference, of Residues, or any others,—which can exist of this description.

Unfamiliar as this mode of approaching the subject may be, there is no doubt that it is the one which would naturally be adopted by any one with the slightest mathematical training. Given a finite number of possible antecedents, with the knowledge that one or more of these was the cause of the phenomenon in question, what other course should we think of adopting than that of marshalling all the possible combinations, and examining their consequences? Before rearranging these results, however, and discussing them under their more familiar logical aspects: and also before enquiring to what extent they fairly represent the actual processes performed by inductive discoverers: it will be well to make a few general reflections upon them as they present themselves in the above simple illustration.

In the first place, then, it may be noticed that when the various antecedents can really be isolated, as they can in the last of our four examples here, the enquiry is not only extremely simple, but it is complete, so far as it goes, at each step. Every antecedent which is followed by the phenomenon is a cause; every one which is not so followed is not a cause. That is, every single observation gives us good and complete information, positive or negative; without the necessity of our having to appeal to any supplementary observations.

Now why is it that we are unable with equal ease to determine a cause when the antecedents cannot be separated? For instance, in the first observation: Why is it that we are prevented from saying that *ABCD*, viz. the simultaneous occupation of all the four rooms, is a cause of the ringing? Simply owing to the assumption, rather tacitly than avowedly made, that by a 'cause' is always to be understood the minimum group of antecedents that will certainly be followed by the effect. This is a council or postulate of moderation, so far as practical purposes are concerned. But for this condition, any one could assign the cause for anything, for he need only invoke the sum-total of all that had happened immediately before, to make certain that he had secured what he wanted somewhere within the wide reach of his net. This would be to repeat Lamb's account of the early Chinese method of roasting pig, by burning the whole house, pigs and all included.

Into this point we need not enter further here, since it has

been already discussed under the head of Causation. It need merely be remarked that, by strictly limiting the number of elements to be included in the Cause, we obtain a larger number of occasions to which we can appeal. If we allow the presence of a multitude of unnecessary items,—unnecessary, that is, for securing what we have resolved on considering to be the effect,—we thereby restrict unnecessarily the number of opportunities of occurrence of that causal connection: we omit in fact many occasions on which that effect really happens.

The reader should notice what exactly is the advantage of taking a plurality of instances in the above problem,—for instance, of enquiring the result of putting together the four combinations *ABC*, *ABD*, *ACD*, *BCD*, in our second batch of experiences; and the six corresponding combinations in the third batch. We there saw that, when *three* elements were taken in a group, we were unable to say what the causes were, though we were able to say that there must be more than one cause; but when *two* were taken together we were able to state precisely what the causes were. The question therefore may be asked, what is the requisite number of such elements to be taken together in order to give us the desired information? The answer is that this is a function of the total number of possible antecedents and of the number of these which are causes. The general formula prescribes that if there be n of these distinct antecedents A, B, C, D, ... to be taken into account, and if m of these can play the part of causes of the phenomenon in question, then the greatest number which can be taken in groups together, with the certainty of thereby eliciting the causes, is $(n-m)$. Thus, in the above example, we found that it was no use taking the elements three together when two of them could play the part of causes; for, in such large handfuls we could not help including both the two causes and something else as well. It was requisite to take the elements two together; and then in some one instance just the two causes, and nothing else, would have to be omitted; with the result of the effect dropping out as well.

The reader must however observe that this single negative instance, in which all the possible causes are just omitted and the effect in consequence fails to appear, need not necessarily

be appealed to. Our work of determination has been accomplished by the time we have reached it. Thus, in the third set of observations in the enquiry above, the requisite facts came out clearly enough from observation of the five affirmative instances. We were able, that is, to determine (1) that there is no single room the occupation of which will account for all the observed occasions of the bell ringing; (2) that the only pair of rooms that will suffice is that of *A* and *B*. This is all that we require, assuming that the cause is always to be understood as the minimum group which will account for the facts. No doubt the reference to the sixth case of occupation, viz. that of *C* and *D* only, confirms this conclusion; for here we have succeeded in putting all the non-effective elements together and find that the phenomenon of a ringing bell does not follow; but the previous instances, taken together, were sufficient to show that neither *C* nor *D* could be effective for the purpose.

II. The example which we have thus discussed in detail seems to be a perfectly fair and appropriate one to illustrate the familiar letter-symbol illustration now so commonly adopted to explain the nature of the Four Methods of experimental enquiry. And in the course of the above discussion we have really had before us all the materials for all those Four Methods. Before reconsidering and regrouping them in accordance with the now familiar scheme, there are two preliminary enquiries into which we are bound to enter. We must ascertain, that is, firstly, under what conditions, tacit or expressed, we have been so far proceeding; and, secondly, to what extent these conditions may be considered to hold good in physical investigations of the ordinary kind.

As regards the requisite conditions, the following seem to be the more prominent and essential.

(1) We assume that the antecedents possess what may be called a definite individuality of their own, in the same way as do the letter symbols, *A*, *B*, *C*, *D*, ... They are regarded as things or objects which can stand apart and be treated separately; so that there is a perfectly intelligible sense in which we can suppose each of them to exist by itself as well as in combination with the others. *A* and *B* can be added on, or removed, without making any difference in the condition of *C*

and *D*. It is clear that the occupation of the different rooms in a house thoroughly fulfils this condition.

(2) It is assumed, again, that any combinations of these antecedents are admissible: at least it is only in so far as this holds good that the method has free scope, depending as it does on the theory of combinations and permutations. We suppose, that is, that *A* and *B* can be taken together or separately, and, in either case, with or without *C* or *D*; and so on with all the other possible combinations. This holds good in the case of the occupation of the bed-rooms, for clearly four of them may be occupied two and two together in six different ways.

(3) Moreover we must suppose that any particular aggregate of antecedents,—say, our *ABCD*,—only admits of subdivision in a perfectly determinate way, corresponding to the letters themselves. It is regarded as a group of individuals in a strict sense of the term. We are not permitted to split the group up at any other points than those obviously suggested. Here again the occupation of the rooms reasonably fulfils such a condition. Given four rooms we cannot make any other arrangements than the 16 which are furnished by one or more being occupied or not.

(4) Fourthly, we take for granted a good deal of extraneous knowledge; knowledge, that is, which was not furnished in the explicit statement of the problem. We assume that our information is complete in respect of the possible causes: that there are just a certain number, corresponding to the four letters, say, with which alone we have to reckon. This assumption is involved throughout. For instance, in our example, it must be supposed not merely that those four rooms are the only ones from which the bell is rung, but that there is no other way, before or during the observations, of causing a ring except by the occupation of one or other of these rooms. The state of things which we postulate, in fact, is not that of a definite amount of knowledge surrounded by an indefinite extent of ignorance. It is rather that of an indefinite extent of knowledge broken by a definite gap of ignorance. The question before us is simply this, Which of a strictly limited number of alternatives is to be accepted?

(5) We need hardly perhaps add explicitly to these con-

ditions that we take for granted the usual assumptions under which the Law of Causation is to be accepted. What has happened once will always happen under similar circumstances; and we take for granted that similar circumstances do repeatedly present themselves. Show that A has once produced X, and we assume that A will always produce X. These assumptions and conditions were fully explained in the chapter on Causation, and need not be further expounded here.

Let us now turn to enquire how far these conditions are actually secured in ordinary physical investigations. We will take for consideration a familiar example which is not unfrequently employed to illustrate the Inductive Methods, viz. that of the *formation of dew*. This is the effect, corresponding to the ringing of the bell in the artificial example, and which we may therefore symbolize by X. As regards this element of the enquiry, there is not much to complain of in respect of its not answering all reasonable requirements. It is not, in fact, at this point that any serious deficiency was to be expected. An *effect*, from the mere fact that it has so far attracted attention as to have given rise to some enquiry as to its cause, is presumably of a tolerably definite description. Moreover, being an effect, it presents itself under the usual conventional restrictions of an effect as contrasted with a cause; and this (as was fully explained in a former chapter) represents a very considerable simplification. We are supposed to have abstracted some one element out of the complex group of consequents, and to be considering only its presence or its absence. We take no account of any accompanying elements here, such as might be symbolized by Y, Z, W, and so forth, as we do in the case of the antecedents.

When, however, we turn to these antecedents we find that there is a very different tale to be told. What, for instance, are the antecedent elements in the case of 'the precipitation of dew'? If asked to enumerate them we should begin with such as these:—(1) moist air; (2) cooling gradually, (3) in contact with a cold surface, (4) under a clear sky, (5) in summer, (6) out of doors; &c. These comprise the principal group of antecedents, present in all or nearly all cases of the occurrence of the phenomenon. To these may be added a number more,

present in some cases and not in others. The list might be continued by such conditions as the following, when these occasional elements are taken into account:—(7) the moisture depositing itself on grass, (8) in an inland locality, (9) on a moonlight night; and so on, as long as we choose to continue our specification.

Now it needs very little consideration to perceive how widely such a group of phenomena as this,—and the example is really rather a simple and suitable one for our purpose, so far as original physical investigations are concerned,—departs from the conditions above enumerated as demanded for accuracy, or at least for conformity with our artificial example and its symbolic rendering. For instance, the various elements which we regard as the antecedents are very far from possessing anything like a definite individuality. All air is to some extent both warm and moist; so that we have not here a simple question of presence or absence, but of presence to a sufficient extent or not. The air, again, constitutes a medium which is everywhere continuous, so that in separating off a portion for consideration we are creating an artificial unity. In fact our A, B, C, D, in such a case as this, are not,—if one may so put it,—*jointed* in certain places so as to leave us nothing to do but to break them off at the right points.

Again; in respect of the admissibility of any combinations of the elements, there is much left to be desired. Owing to the intricate mutual interaction of almost all physical causes, it is simply impossible that there should be an alteration in one antecedent without some corresponding alteration in the others. We can turn a man out of one room in a hotel without disturbing his neighbours, just as we can remove the letter A from a row without touching any of the others. But we can hardly alter the moisture of the air without also altering the temperature, and contact with some cold body is necessary, as a rule, in order to lower the temperature.

Again; in our artificial example, we found that the assumption that a given group, such as that indicated by $ABCD$, could be subdivided up to a certain extent and no farther, was reasonably fulfilled. The occupation of a single room is a sort of unit, which, for the purpose in hand, really does not admit of any

further subdivision. But the closer we look into any physical phenomenon, the more apparent does it become that such separations as we make here are mainly the resultant of the practical necessities of the moment, and of the words and phrases which we happen to have at hand. There is generally no obvious limit here up to which, but not beyond, the process of separation into elements can be carried.

As regards our fourth assumption, the reader can readily perceive what sort of warrant for it there is, in any enquiry of a physical nature. The hotel servant might undertake to say that there were just four rooms to be taken account of, and no more. But what sort of a task should we have before us if we set about to enumerate, not all the possible antecedents effective in the production of dew, but merely those with which we were acquainted and which might possibly be effective? It is here, I think, that the common accounts of the logical manuals are most inadequate. They scarcely make the reader realize sufficiently what a large amount of preliminary knowledge he must be supposed to bring to bear upon the observation or experiment which he has in hand. It is on this ground,—on the ground, that is, that much of the implied knowledge appears, so to say, on both sides of the equation, and can therefore be practically dispensed with,—that the Method of Difference acquires the prominent position which it occupies as regards convenience and certainty of inference. So far as regards the Methods in general it is sufficient to say that we must suppose our observer to approach the enquiry equipped with the general preliminary knowledge that there are a certain number of possible antecedents which, and which only, need be taken into account, and which he can indicate as A, B, C, D....All the others are either ineffective in themselves, or, if effective, can be allowed for or excluded from the sphere of our observations and experiments.

The best way of explaining and illustrating this will be to throw our physical example at once into the symbolic form. It may be done as follows:—

Moist air; cooling, in contact with cold surface	on grass	outdoors	in summer	sky clear
A	B	C	D	E......
x	p	q	r	s......
Formation of dew	grass wetter	outdoors	Summer	sky clearer

The reader will notice at once that this is a somewhat different way of representing the causal relation than the customary one of putting it down simply in the form,

$$ABC,$$
$$abc.$$

The above plan has been adopted purposely for the sake of emphasizing the difficulties which the complication of physical phenomena throws in our way. For one thing, the reader will notice that what we have marked by A is decidedly a somewhat artificial unity. This component requires several clauses to describe it, and is naturally viewed as a compound of several different elements. But for the purpose in hand we have to make a unity of it, since the absence of any one of its elements would be fatal to its efficiency. Another small point to which we may call attention is the employment of distinctive letters of the alphabet for the causes and effects. The common notation, employing as it does the same letters, with the only distinction of making these capital or small, inevitably suggests such an arrangement as we found actually did prevail in the case of the bells in the hotel, that is, that there were a number of quite distinct cause-elements, each connected with a correspondingly distinct effect-element, so that all which we had to do was to sort them out and assign each to its appropriate relative. It is well to keep clear of any suggestion of this kind, and therefore to write down the effect-elements, not as *bcde*... but as

pqrs...; and thus to avoid all suggestion that *B*, *C*, *D*,... produce *p*, *q*, *r*,... respectively, in the same way that *A* is considered to produce *x*. As a matter of fact, when we look at these effect-elements, that is, the concurrent elements which go along with the consequent in the causal relation, we find that a certain number of them are merely a continuation of the same element in the antecedent. For instance, what we mark as 'summer' and 'outdoors' must just be written down again unchanged as consequents. Others again, represent a certain amount of change. For instance, the 'grass' in the antecedent was dry, that in the consequent is wet: the 'sky' in the consequent is probably clearer than that in the antecedent, by the very fact that some of the moisture has been transferred to the surface of the ground. Other elements again may have been of such an extremely transitory character that they may have disappeared, and either have left no perceptible trace whatever of their existence, or one which in the shape of a consequent bears no apparent likeness whatever to its assigned antecedent.

The above symbolic statement, therefore, with its equivalent verbal rendering, represents *one* of those sets of observations or experiments, of which a plurality is presupposed in all our investigations. If it be asked what are the other observations which make up the set, we can easily enough give examples. For instance, we must exchange the 'grass' for something else, since it is conceivable that the dew might be of the nature of a perspiration emitted by the leaves. So we take a corresponding instance on 'gravel' instead. Again, it might be the case that rain was in the habit of falling even when the sky was clear; accordingly for 'outdoors' we substitute a corresponding experiment indoors. In this way we procure a number of instances which we may represent thus;—

ABCD...	*AGCD*...	*ABHD*...
xpqr	*xtqr*	*xptr* ......

as well as a number of others in which *A* is absent. And these form the sum-total of the materials upon which our various Inductive Methods of Enquiry have to operate.

Supposing that these materials were set before any one who had but a slight acquaintance with the details of any physical science,—more especially if he were familiar with the

elementary algebraical processes of combinations and permutations,—there can be little doubt, I think, that he would try to set to work in the way in which we treated our artificial example. Regarding the problem as a purely formal one, in which some one or more of a number of antecedents were the effective ones, and in which these antecedents admitted of being compounded and dissociated at will, he would treat the problem in the sort of way in which we treated the example of the bells. Since there are a finite number of elements to be taken into account, and therefore a finite number of combinations to be grappled with, he would naturally like to spread them all out before him by a complete analysis. And in selecting among these results such as were best for his purpose, he would naturally aim in every case at discovering those in which *single* antecedents were given, owing to their extreme simplicity and effectiveness. Failing the possibility of getting hold of any one of these by itself, he would try for a set in which two antecedents only were present, and so on. That is, he would, when guided by formal considerations alone, not rely on special 'Methods' of any distinctive character, but make use of any combinations whatever that presented themselves, giving the preference in every case to those combinations which were the smallest in their aggregate amount.

III. When however we turn to the actual facts of nature we find a very different course forced upon us. Here as elsewhere, formal propriety and convenience have to give way to the exigencies under which we are practically obliged to work. We are employing formal methods, as indicated by the use of our letter symbols, but we soon find that the conditions really suitable to a purely formal method are greatly lacking, and we have accordingly to modify our practical processes in accordance with the facts which we find about us. It is this necessity which drives us to the adoption of the well-known Four Methods, and assigns to them their comparative importance. We will therefore proceed to discuss these in the order of their comparative value and effectiveness.

(1) The Method of Difference. We saw that one serious departure from those simple conditions of the problem which seemed naturally suggested by the employment of a limited number of letter-symbols, lay in the fact that in natural, as

distinguished from artificial, circumstances, the determining antecedents were often quite indefinite in number; and, what was worse, that it was impossible to draw clear partition lines between them. There seems one way, and only one way, of surmounting this difficulty. What we must do is to contrive a method by which all these indefinitely numerous and perplexing elements shall appear, so to say, on the two opposite sides of the equation, and thus be made to balance each other. We need not then trouble ourselves to determine them at all. The way of effecting this is to take two instances,—for two will suffice for the purpose,—not from the same rank or order of complication as regards the number of antecedents (recur to the example on p. 405), but from two successive ranks. We select an *ABCDE*... for one of these, and a *BCDE*... for the other; where the latter selection contains one element fewer than the former.

If it be enquired how this can best be done, we find ourselves again practically conditioned by inevitable physical surroundings, which confine us to one way of effecting in practice what is so easily and variously effected in the region of symbols. Our only resource is, not to try to find two things exactly alike in all their innumerable characteristics, but to take one and the same thing or event, as nearly as possible at two consecutive instants. There is really no other way open to us; for, by supposition, we do not know all the antecedents, and therefore we cannot certainly secure them by the most painstaking selection.

Under these conditions it becomes really possible in many cases to secure a pair of instances, symbolically represented by *ABCDE*... followed by x, and *BCDE*..., not followed by x. By a comparison of these two we conclude quite soundly that since every one of the antecedent elements, except *A*, is present in both cases, this single element whose presence and absence correspond to the presence and absence of the effect, must be what we call the cause in that case. And therefore, generalizing in accordance with the Law of Causation, we conclude that it will always be the cause of the same effect under all similar circumstances. That is, we have ascertained the Cause of the effect under consideration.

The validity or conclusiveness of this method seems there-

fore to be absolutely unimpeachable. But unfortunately it lies open, so far as theoretical considerations apply, to a quite fatal objection from another side, viz. that of its applicability[1]. The dilemma is obvious. If we did know that A was the cause, why resort to the method at all? And if we did not know this, how did we just hit upon A, out of all the innumerable antecedents, in order to exclude this alone from the second experiment?

There seems really to be no means of evading the objection except that of frankly admitting that this Method of Difference is not in reality a Method of Discovery, but solely one of Verification. We must be supposed to appeal to it as a final resource, in order to test whether some assigned antecedent which has in some way or other been suggested to us as a probable cause, is really such. It may be that we have first appealed to some one of the other methods, to be presently described, which really are more in the way of agencies of discovery, but are correspondingly less conclusive in their results until these have been verified. Or it may be,—as often is the case in physical problems,—that we come to the consideration of the particular question before us with a good general knowledge as to what most of the different antecedents are capable of doing, so that only a finite and indeed very limited number are left as needing investigation. These few may then be discussed in turn by this Method of Difference.

We have spoken of the method as being certain in its operation, if we can surmount the primary difficulty of knowing how to apply it in any given case. This certainty depends of course upon conditions, which as a rule are not very difficult to secure, and of which the following seem to be the chief.

(i) We must take it for granted that, when we thus repeat the whole group of antecedents, save one, we shall not find that some other circumstance has just slipped in which might itself be influential in bringing about the effect. As, by supposition, the antecedents are probably too numerous for us to be acquainted with them all, the only way to secure that this does not happen is to take care that the two successive occurrences, or moments of occurrence, which we bring into comparison with each other, shall be in close proximity. In other

[1] That is, when using it, as we commonly do, to determine the cause of an effect, rather than the effect of a cause.

words our experiment must not be suffered to extend during too long a time.

This caution is preeminently necessary in the case of social and political reasonings, in which indeed the process of evolution is so slowly carried out that the Method in question becomes almost impossible of application. Every one can see what a fertile source of fallacy and sophistry we have here. A new law is enacted, say, or some act of policy is carried out. This closely resembles an experiment,—in fact it is one,—and if such experiments could be carried through with the promptness and strict limitation with which we can perform them in many physical phenomena, there is no reason why their results should not be equally conclusive and satisfactory. But when we are dealing with causes which take years to do their work the state of things is widely different. It is absurd to suppose that all things else have stood still in the meanwhile. If we were to abolish all licences or other restrictions on the sale of alcoholic liquor, and were to find five years afterwards that habits of intoxication had increased, this might be a case of cause and effect. But we should hardly feel as much confidence about the fact as we feel when a lead manufactory turns its foul water into a lake and it is found next morning that all the fish are dead. The tacit assumption that nothing else has happened amongst the phenomena except the change which we have introduced, and its corresponding effect, can only be permitted so long as we take care to be very prompt in carrying out our experiment.

When we take this precaution we have the sort of security, justified by the Theory of Probability, upon which every one instinctively relies in daily life. If I shut my door sharply and the window upstairs instantly afterwards is heard to rattle, it is just possible that something else than the wave of percussion in the air should have happened at the same moment to cause the noise. But the closer the sequence the smaller does such a chance become. Hence the necessity, if the Method of Difference is to be trusted, that our artificial interferences should be sharply determined, and that the moment at which the second determination is made should be as closely proximate as possible to that of the former.

(ii) The second precaution is of a purely practical character, and is one for which there is no counterpart in the symbolic procedure. The trained physicist however knows only too well how extremely difficult it is to carry out the demand sufficiently. The requirement is merely this: that the process of omitting or inserting A shall not in itself bring about other collateral or incidental effects beside the contemplated step and its consequences. But theoretically, the laws of nature being what they are, it is impossible to preclude all such aberration from our aim as this implies. Take the instance of gravitation. We commonly suppose that when we have put a weight into one pan of a pair of scales we have done nothing more than this, or can at any rate by due caution succeed in doing nothing more. But, if we exact the utmost rigidity of conditions, we easily see that we have done a great deal more. Our bodies are heavy, and therefore the mere approach to the machine has altered the magnitude and direction of the resultant attraction upon the scales. Our bodies are presumably warmer than the surrounding air; accordingly we warm and therefore lighten the air in which the scales hang, and if the two scales and their contents are not of the same volume we at once alter their weight as measured in the air. Our breath produces disturbing currents of air. Our approach affects the surface of the non-rigid floor or ground on which the scales stand, and produces another source of disturbance; and so on through the whole range of the physical world. Every action we perform in the material universe sets in work forces whose ramifications are incalculable, and whose effects have no assignable termination.

The avoidance of these disturbances, or departures from the strict adherence to the assumptions of our Method, is therefore a matter of judgment and sagacity. The extent to which precaution is called for in any particular experiment depends entirely upon the degree of accuracy to which in the case in question we propose to carry our observation. Many precautions, for instance, which it would be ridiculous for a tradesman to adopt if he were weighing out a pound of sugar for a customer, are just the things which it would be little short of criminal for an analytical chemist *not* to adopt, if he were set to determine whether a given sample of water were

pure. And precautions again which would suffice for this latter may again become insufficient when some original investigator is carrying out an extremely refined course of experiments in his laboratory. All accuracy in these matters is a question of degree, to be determined by the end we have in view, and strictly regulated by the necessities which the attainment of that end reasonably demands.

Take a single example from an actual course of experiments. When Prof. Sir G. H. Darwin and his brother were endeavouring to measure the Lunar disturbance of Gravity[1] at the Cavendish Laboratory an extremely delicate pendulum was employed. So delicate was it that it almost defeated its purpose by registering innumerable minute disturbances, of which, whilst many could be accounted for, many others baffled all explanation. Amongst the former was this. In approaching the instrument in order to observe its reading, the surface level of the stone basement floor on which the instrument stood was deflected by the weight of the observer. Nay, as he stood to take a reading, the difference produced in this way by his merely shifting his weight from one leg to the other was perceptible; so it became necessary always to observe the reading by a telescope from a distance, or to adopt some equivalent plan. Now of course disturbances, more or less of a similar kind, are actually brought about whenever we have a letter weighed at the Post Office. But as it is not considered that the extra pence at stake are worth the trouble of deciding the weight of the letter to such a point of accuracy, we are content to let this source of inaccuracy enter, and therefore we use instruments too coarse to indicate it when it does enter.

(2) There is a modification of this Method of Difference, which possesses many of the same advantages. It does not, it is true, belong in full strictness to that limited view of the subject to which we are confining ourselves in this chapter, inasmuch as it is not a merely qualitative method, but partakes to a certain extent of the quantitative character. Since, however, it has received a special name by Mill,—that of Concomitant Variations,—and has been commonly accepted in most recent

[1] Report of the Brit. Association, 1881: p. 109. Since fully described in his work on the *Tides*.

treatises on the subject, it will be best to notice it in this connexion.

The reader will notice, then, that throughout the above discussion we have supposed the variable antecedent A to be capable of no other variation than that between mere presence and absence. In very many cases however it may happen that we find it impossible altogether to get rid of A; or, at any rate, that we wish to form a conclusion without the trouble of doing so. Hence the question arises whether partial absence of the suspected antecedent will serve the same sort of purpose as entire absence.

Is it true then that if the entire removal of A will entail that of x, a mere variation in its amount will entail a 'concomitant variation' in that of x? Plausible as such a statement may seem, it demands some consideration and reference to specific examples. In the first place, then, there is the preliminary difficulty that in a great number of instances,—of which our example about the bells is a case in point,—it is not very clear what is meant by speaking of 'more and less': at least if we were to apply this expression to the occupation of a room we should have to put a rather special interpretation upon it. This however is more of the nature of occasional inapplicability of the method than of actual failure.

Confining ourselves to really applicable references let us take a few examples:—There are a quantity of cocks to the various gas-pipes in a large building. I turn one of these, and all the lights in a certain gallery instantly go out. This is the usual method of Difference. But if I merely turn the cock half way round, two or three times, and see these same lights rise and fall as I do so, the same conclusion would be just as confidently drawn. So far the method seems quite sound. But again: instead of gas-pipes there are conducting wires leading to a set of electric lamps. I cut one wire through, and a certain group of lights instantly goes out. This again is clear and conclusive. But if I cut the wire half way through, probably no change whatever will be perceptible, because the full current will equally pass through the thinner wire. Or, suppose there is an open drain running through a court which is much infested by fever. I close the drain entirely, and the fever ceases. This is the Method of Difference. I half close it: is there much hope

that the fever will be checked? Such a list of varying and conflicting answers might be indefinitely added to.

It will doubtless be objected here that some of these instances are not fair ones, because we are not varying the really effective causal elements, but only certain conditions of them. Vary the *current*, it will be said, instead of merely varying the thickness of the wire which conducts it, and the lights will vary at once. Diminish the number of the poisonous germs emitted by the drain, or the number of human organs into which they can enter, and we shall pretty certainly diminish the number of attacks of fever.

This objection is perfectly sound in itself: in fact any other conclusion would be inconsistent with the doctrine of Causation in its more stringent form. As we have already insisted in a former chapter, it is scarcely possible that any element whatever in the antecedent group should be altered without entailing some corresponding alteration in the consequent. But as regards the particular order of enquiry with which we are here concerned, such an objection as this does not seem to be quite to the point. No claim of this kind,—namely that we were dealing, not with the true causal elements, but only with certain conditions accompanying these,—was advanced, so long as we kept ourselves to the Method of Difference in its simple form, and supposed A to be entirely removed. It was only when we began to find that the partial removal of A did not always bring about the partial removal of a, that the objection came to be raised that this A was not really the proper element to deal with.

Attention is here directed to this particular point because it is very important that the student should clearly recognize that these Inductive Methods, which play so important a part in our logical treatises, are not of a rigidly scientific character. They belong rather to what may be called the plane or level of popular enquiry. For the signification of this distinction the reader must refer to the chapter on Causation, in which the different aspects of that Law were clearly explained. These Methods are nothing more than practical applications of the Law of Causation when this is interpreted in a popular scientific form. As we saw, the Law only became practically useful, or even available, when based upon this reasonable but humble level.

The Method of Concomitant Variations, as above remarked, is nothing more than a modification of the Method of Difference. It concerns itself with the partial presence and absence of a phenomenon, instead of its complete presence and absence. If we were able to say, with any attempt at exactness, *how much* of this presence there is in any case, our method would no longer be of that merely qualitative kind to which we are now confining ourselves. And nothing short of such exactness will satisfy the scientific thinker as an ultimate aim. But as soon as an attempt is made to reach this higher standard we get into the province of quantitative determination, which is beyond our present scope. It will suffice to say here that this slight quantitative modification of the Method of Difference, in which we merely say that there is more or less of the antecedent and of the consequent, without attempting to decide how much more, is very useful, and is freely and confidently appealed to in practice.

The reader will be able to gather, from the remarks that have now been made, what are the general characteristics of this Method of Difference. It is with moderate and reasonable precautions a conclusive method; but as against this merit must be balanced the fact that it is only under decidedly exceptional circumstances that we can expect to be able to appeal to it. It presupposes, as has been already pointed out, that we have previously obtained some clue as to the nature and whereabouts of the desired cause, so that what is still left to be done is not so much to discover as to test. And, in intimate connection with this condition, the method also presupposes that we have some considerable control over the facts under investigation. That is, it is a distinctly *experimental* method. Not only must we have some clue as to what the cause is, but we must also possess some power to produce this cause at will, or we could never put the method in practice. In fact, as we saw, it is not only necessary to be able to produce the cause, but to produce it with promptness and definiteness; as otherwise disturbing agencies will begin to get into play. Such requirements as this, it need hardly be said, greatly limit the range of applicability of the Method in question. This subject will be recurred to again in a future chapter, when we come to discuss the nature of Experiment and its distinction from Observation.

(3) The Method of Agreement. This is generally phrased in some such way as this:—If any two or more instances of the occurrence of some phenomenon (the effect) have one element, and one only, present in common among their antecedents, this element may be presumed to be the cause of the phenomenon. And it is symbolically illustrated in some such way as this:—

$$\begin{array}{ccc} ABCD & ABEF & ACEG, \\ abcd & abef & aceg. \end{array}$$

Here a is the effect whose cause is sought; and since A is the only element among the antecedents which is found in all the instances, we conclude that A is the cause of a. This account of the matter will be found expounded with an abundance of popular and scientific illustrations in Mill's work, and in most other treatises on Inductive Logic.

The chief objection to this mode of procedure is that its validity rests upon the assumption that the phenomenon whose cause we are investigating has only *one* Cause. Now it has been abundantly shown that Plurality of Causes has to be admitted as a consequence of our interpretation of the Law of Causation, and it seems therefore inexpedient to ignore its existence even temporarily. Its admission indeed has such a serious bearing upon the validity of the Method of Agreement that it seems desirable to depart from the customary style of treatment here. Instead of starting with the supposition that causes are unique, and showing how convenient our method would then be, we will commence the study of the method on the assumption that the possible causes may be more than one. It will require some attention to grasp the exact value of such proof as the method can afford on this assumption.

Recur for consideration to the example discussed on pages 404—7. We saw that when $ABCD$ are all found together x is also found to occur: does this give us any reason to assume that A in particular is the cause of x? We are of course taking for granted,—what we saw to hold true in our first and artificial example,—that these four elements of the antecedent, A, B, C, D, are separately intelligible and possible, and that for aught we know to the contrary there is no reason why any one of them should not be the real cause. In this state of things we have nothing else to appeal to but the

Doctrine of Chances. In accordance with this doctrine we may accept the ordinary view, and say that if there is only one cause it is just one chance in four that A is the cause. And if, again, we were asked why we suppose that there *is* only one cause of x, instead of two, three, or four, we should really have no other reason to give than to fall back upon what is sometimes called the Law of Parcimony, by saying that it is always best to assume as little as possible, provided this will account for the facts.

Now take the next case in order, of those discussed on p. 405, by supposing that we have ABC, ABD, ACD, BCD, presented as instances in which x is found. They have no element in common; therefore there must be more than one cause at work. But, so far, we have not the slightest clue as to which two out of the four are the effective elements. The Theory of Probabilities, when appealed to, will give the faint clue, that if there are known to be only two causes then the chances are one in six that these are A and B. But when, as before, we ask why there should be only two rather than three or four, we can give no better reason than that this is the simplest assumption. The statistical basis for any more scientific answer is entirely wanting. It is impossible to assign the relative frequency of prevalence of events with one cause, two causes, three causes, and so on.

It is only when we get a stage further that this Method, of Agreement pure and simple, begins to offer us any help. As the point is an important one, but is often overlooked, we must ask the reader's careful attention to the inferences which can be drawn from what may be called the affirmative instances, that is, from those in which the presence of the antecedent is followed by the presence of the consequent; for it is these which form the materials of this Method of Agreement. When we took the elements two together, we saw that there were five combinations favourable to the result, viz. AB, AC, AD, BC, BD. To have all these before us is to be in possession of complete knowledge of the materials possible in this particular stage of analysis. Do they suffice to determine the causes? As before, we do know one thing for certain, viz. that there must be more than one cause at work; since no one element is present throughout. And, more than this, if we know that

two causes only are to be admitted, then we can easily show that they must be these two. The suggestion, for instance, of *A* and *C* would not answer; for the effect occurs when both of these are absent, viz. when *B* and *D* are the only antecedents. And similarly with any other pair than *AB*.

The question then strictly takes this form: What are the chances in favour of two causes over three? If we are confined to two, these, we have seen, must be *A* and *B*. But if we are at liberty to invoke three, then *any* three will answer the purpose, as the reader will easily see by looking through the complete list. But why is it more likely that there should be two than three, and how much more likely is it? To the latter enquiry no answer whatever can be attempted; for statistics on the subject are entirely lacking. To the former enquiry the answer is sometimes given in the form of an old saying which announces that Nature always works by the simplest means. There is really no need to discuss such a maxim as this at the present day; but if we change the advice into a suggestion to ourselves not to take unnecessary trouble, and therefore always to begin with the simplest assumption which will answer our purpose, there is a good deal to be said for it. And taking the view that we here take,—that is, that the Method of Agreement is nothing more than a preliminary process of sorting out our supposed causes, with the view of testing them afterwards by the Method of Difference,—it is a complete answer. Whether or not Nature is more in the habit of employing two causes than three,—in the conventional sense[1] in which, of course, we here understand a 'cause',—it is impossible to say: but there can be no doubt that it is less trouble to start with the narrower supposition and to proceed to verify or confute this first.

In the above investigation we have supposed the number of possible alternatives in the antecedent to be so small that it was quite practicable to take them all into account. This was the case with the hotel bells; and the sort of alphabetical illustrations given in text-books may suggest that instances such as this are normal. We need hardly now however remind the reader that when we are dealing with natural phenomena

[1] The assumptions under which 'Plurality of Causes' is possible were explained in Chap. II.

we must look for a very different state of things indeed from this. We must allow for the possibility that the number of possible alternatives is large; and, when this is so, the number of their combinations will soon become enormous.

For instance; see what could be concluded from the observation that x occurs in the following cases:—

ABCDE *AEFGH* *AFHIK* *BCGIK* *BLMNO*

This can hardly be considered a very complicated example as compared with what might be actually encountered. The number of antecedents introduced into each group is only five, and the total number of distinct antecedents is only fourteen. But the slightest consideration will show the exceedingly precarious nature of any inferences we can draw from the materials before us. One thing of course is plain at once. Since no one element is present in all the groups there must be more than one cause at work to produce the effect. It is almost equally plain that there is one pair of antecedents, and one pair only, of which one or the other member is present in every group; accordingly, if we are sure that there are only two causes to be admitted then these must be the two: i.e. *A* and *B*.

This assumption, somewhat arbitrary as it is in any case, is much more so here than it was in our example of the bells, on p. 405. When there were only four alternatives in the question, to invoke three of them when we could put up with two, seemed rather wanton. But, when there are fourteen, to insist upon confining ourselves to two when we could do the work with three, seems more like an economy dictated by our own convenience than by the probabilities of nature. The three elements *B*, *E*, and *F*, are perfectly capable of constituting the desired Plurality of Causes; and so are a variety of other groups which we might select out of the fourteen. The preponderance of evidence against any one of these slightly more complex suppositions is very slight.

It must be noticed that our uncertainty is greatly increased here owing to the fact that the number of instances with which we can reasonably expect to deal is such an exceedingly small proportion out of the possible total. It was shown above that when we had only four elements to deal with

we could easily suppose ourselves in possession of all the favourable combinations, and that with these before us we could gather a certain presumption in favour of A and B as causes. But when we have fourteen of these elements on hand, and these grouped together by fives, it would take over 2000 combinations to exhaust the possibilities. As it is out of the question to examine all these we must judge them by sample. Can we hope to get a fair sample? This depends upon the number of cases involved. When we said, just above, that if there were only two causes these must be A and B, we were of course speaking only on the warrant of the evidence before us. This evidence consisted of five cases: the reader can give some sort of guess as to the value of a sample composed of a selection of merely five out of two thousand. It is quite possible that if we had enlarged our experience until we had 50 or 60 cases instead of only five, we should have found that the plurality of causes which it was simplest to invoke would now become decidedly more numerous. We might have found, for instance, that the simplest possible assumption which would account for the production of x was afforded by supposing it to come from one or other of some half-dozen distinct causes.

We seem therefore to be driven to the following result. When we appeal to the Method of Agreement pure and simple, taking care to recognize the possibility of a Plurality of Causes, any conclusion we may reach is highly precarious. Say that x occurs (as above) with $ABCDE$, $AEFGH$, $AFHIK$, $BCGIK$, and $BLMNO$. We can conclude that *if* these are all the possible favourable instances (a remarkably bold assumption in most cases), and *if* there are no more causes to be invoked than the minimum number which will do the work (a remarkably arbitrary assumption) then A and B are what we seek.

The utmost value therefore which we can venture to assign to the simple Method of Agreement is, that it should suggest to us the directions in which it will be convenient to resort to experiment or some other really conclusive test. At least this seems all the value which should be assigned to it in instances which can be described as of a natural rather than an artificial character.

(4) During the last few pages, in which we have been

discussing the Method of Agreement in its simple form, we have been engaged in determining what can be discovered by the consideration of affirmative instances; that is, of those in which the cause and the effect were both present. We must now see what can be done by taking the precaution to examine negative instances also; that is, those in which the cause and the effect are both absent. It will be found that the reference to these negative instances immediately puts matters into a very different light, and that in fact it may enable us to convert a bare presumption into complete certainty. It is indeed obvious that any experiment in which we can succeed in grouping together at once all the false claimants to causation, and can show that their united efforts are unavailing to produce the phenomenon, is conclusive against the claims of any one of them.

We shall do best by beginning as before with the simpler cases and advancing to the more complex. The reader will remember that, when we were dealing with the bells at the hotel, we saw that, taking the four antecedents two and two together, it was observed that the bell did ring when only *AB*, *AC*, *AD*, *BC*, or *BD*, were occupied; but that it did not ring when only *C* and *D* were occupied. Now we saw that the affirmative instances established the existence of two causes at least, and consequently raised a presumption that there were only two, namely *A* and *B*. The moment this result is tested by the negative instances such a presumption is raised to certainty. We find that neither *C* nor *D* can avail anything to the purpose, and therefore we know that both *A* and *B*, and they alone, must be invoked at one time or another to account for the facts.

We must now proceed to test this appeal to the negative instances, as we have already tested the appeal to the affirmative, by seeing how far this will avail to carry us when we choose an example which makes a slightly closer approach to the actual complexity of nature. Instead of confining ourselves to such a small number of antecedents as will leave it in our power to examine in turn all the combinations, we will take a case where the antecedents are somewhat more numerous. The example on the last page but one will answer our purpose. Add on a couple of negative instances to the affirmative ones considered there. Thus,—

Affirmative: *ABCDE, AEFGH, AFHIK, BCGIK, BLMNO*; all followed by x.

Negative: *CFHIK, CFKMO*; not followed by x.

Here the negative instances at once enable us to effect a considerable simplification. They exclude in fact seven of the otherwise possible contingencies, and leave only seven behind as admissible. We are still far from being driven to accept A and B as the only causes in question,—the exclusion has not been carried far enough for this,—but we have advanced some way towards this result, for we have largely reduced the admissible antecedents. For instance, we saw before that, amongst other suppositions, there might have been three causes operative, viz. B, E, and F. This supposition is excluded now, since F is negatived, and B and E are not sufficient by themselves. There are of course plenty of other suppositions open to us. We might, for instance, suggest that A, G, and L, are the really operative antecedents. If we reject these it is only on the ground, before noticed, that such an assumption is a less simple one than that of A and B, and therefore less likely, or at any rate less convenient.

It appears therefore that it is the negative instances which are really decisive, inasmuch as they clear the ground for us. Every antecedent which we can thus eliminate represents something finally and unconditionally removed. When this work has been gone through, we must proceed to examine the affirmative instances and interpret them to the best of our power. The conclusion which can be elicited from them depends upon the extent to which they are properly representative. If the proportion of actual affirmative combinations before us is small compared with the total possible number, our conclusion must be considered very hazardous. If this proportion is large, our conclusions acquire considerable force. If it is complete, both as regards the affirmative and negative side, then our knowledge is accurate and decisive. There is no need, under such circumstances, to appeal to the Method of Difference, or otherwise to verify the conclusion, and the only relative inferiority of such a method consists in its extreme cumbrousness.

Some such Method as that now before us is, presumably,

what Mill had in view when describing his "Joint Method of Agreement and of Difference". But the title seems somewhat of a misnomer[1], and his mode of describing its nature appears to leave a good deal to be desired in respect of accuracy and completeness. As regards the name; the essence of the Method of Difference consists in the fact that dealing, as we generally do, with what may really be called the same phenomenon twice in succession, the only difference consisting in the introduction of a perfectly determinate change into it, we can grapple with the presence of an *indefinite* number of antecedent elements; for these vanish, so to say, in the act of subtraction, and leave no opening for error. But there is nothing of this sort in the method now under consideration. We must suppose the antecedents to be finite in number here. We have no means of lumping them together, but must deal with them separately, and accordingly the examples of the handbook and the lecture room give very little idea of the attitude in which we stand towards those presented in nature.

Mill's statement of his Joint Method is as follows,—"If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance: the circumstance in which alone the two sets of instances differ, is the effect or the cause or an indispensable part of the cause of the phenomenon". It is clear that before any such statement as this can be accepted we must take for granted a considerable amount of extraneous information, that is, information not contained within the terms of the problem itself. We must take it for granted that we are talking only of *relevant* circumstances, and that we have a fair knowledge of what are relevant circumstances. In any literal sense of the terms it is absurd to speak of two instances agreeing *only* in the presence or absence of such and such characteristics; for any two instances whatever that we might happen to select could not fail to agree in many points of presence, and must agree in simply indefinitely numerous points of absence. It must be understood, therefore,

[1] A better name surely would have been chosen by entitling it simply the Method of Exclusions; a term for which we have the historic sanction of Bacon's authority.

that the 'absences' are to be confined to those circumstances which have been actually present in the affirmative instances, or were at any rate set before us at the outset as elements of the problem to be reckoned with.

Jevons' rendering,—it must be remembered that he is quoting from, and closely following, Mill here,—does not seem any better in this respect. His statement is as follows (*Elementary Lessons*, p. 246):—

"The Joint Method of Agreement and Difference is similarly represented by,—

Antecedents.	*Consequents.*
ABC	abc
ADE	ade
AFG	afg
AHK	ahk
......	...
PQ	pq
RS	rs
TV	tv
XY	xy

Here the presence of A is followed, as in the simple Method of Agreement, by a; and the absence of A, in circumstances differing from the previous ones, is followed by the absence of a. Hence there is a very high probability that A is the cause of a".

I cannot understand this rendering. If the absence of a letter here is meant to indicate,—as is presumably the case,—the known absence of the corresponding cause, then it is clear that the members of the second set of instances have a great deal more in common than the mere absence of A. For B, C, D,... are also absent from them all; in fact the whole second set of instances agree in displaying throughout the entire absence of every element in the first. Mill's verbal statement of the Method, or Principle underlying it, is therefore widely departed from. And if the omission of a letter symbol does not mean the exclusion of the corresponding Cause, how do we know that A *is* absent from the second set of instances?

Surely, to quote Jevons' words, what we want is not "the absence of A in circumstances *differing from* the previous ones", but its absence under circumstances as much as possible

resembling them. The introduction of a set of letters taken from the latter half of the alphabet, for our negative instances, suggests the selection of phenomena widely remote from those appearing in the affirmative instances. Nothing having been said as to the limitation of the field over which they are sought, it would be scarcely a parody of the procedure to say that if *a* stood for the smoking of a chimney and *A*, *B*, *C*, *D*, *E*... for the tight fitting of the door and window, the north wind, fog, &c.; that *PQ* indicated that the price of stocks was high, and *RS* that a parliamentary election was in progress[1].

Surely what we want is something of the following kind. Let x be some phenomenon in regard to which eleven antecedents, say *A* to *K*, are to be taken account of; and suppose that we have collected the following sets of affirmative and negative instances:—

Affirmative.	*Negative.*
ABCDE	*BCFG*
ADEFG	*DEHI*
AFGHI	*FGJK*
AHIJK	*HIDE*

It is clear that *A* is the only element, of the given lot of eleven, which is always present in the former set of instances, and the only one which is always absent in the latter set. If we knew for certain that there could be only one cause, then clearly *A* is that one. So much indeed is established by the affirmative instances. What the negative instances do is to disprove, one after another, all the alternative causes other than *A*. It might be that *A* was not a cause at all, but that *B*, *D*, *F*, and *H* had respectively been at work in producing x in the four cases in question. The negative instances disprove this, however; and since they take account of every one of the ten letters,—or cause symbols,—*B* to *K*, and show that no one of these is operative, we are led to conclude that *A* alone is the cause which we are seeking.

[1] A joke will sometimes bring out a point more forcibly than a serious illustration. Is not the answer ascribed to the schoolboy, when describing the uses of pins,—'that they saved thousands of lives every year, by *not* being swallowed',—a perfectly fair and relevant instance of an "absence" such as is alluded to above?

In the above example we have supposed that the negative instances cover the whole ground after *A* has been omitted, but of course in practice it is to the last degree unlikely that we should be able to get more than a small proportion of the complete assortment desirable.

The general conclusion then to be drawn in reference to the nature, value and certainty of these methods seems to be the following. We must always assume a considerable amount of preliminary information as to the nature and limits of the field over which the cause is to be sought. That is, the claimants for that post must be supposed to be finite in number, and to have all had their names previously submitted to us, so that we have merely the task of choosing amongst their respective qualifications. Indeed we must assume more than this; for unless the possible causes are extremely few in number,—far fewer than they can well be in any physical enquiry,—so that all their combinations can be taken into account, we must take it for granted that we have some indications given to us as to which are the serious claimants whose qualifications only have to be carefully tested. If more than a very few are introduced the combinations to which they give rise soon become quite unmanageable in number.

Having got the possible causes thus before us we proceed to apply our methods to them. We begin with the Method of Agreement, helping this out as far as possible by Exclusions, that is, by the so-called Joint Method. The latter serves to clear the ground, by eliminating a certain number of the otherwise admissible causes; and then the former will give a presumption in favour of one or more of the remaining ones. These latter we must then proceed to test by direct experiment or by deduction. If the nature of the subject-matter is such that we can resort to an experimental test we may then appeal to the Method of Difference. If not, we must try what we can do by deductions from already known and admitted generalizations.

CHAPTER XVIII.

STANDARDS AND UNITS.

(1) PHYSICAL.

UP to the present point our discussion has been mainly, if not entirely, of a qualitative kind: we have now to take the important step which carries us on to quantitative considerations. The fact of being able to take this step is the main characteristic which distinguishes the scientific from the merely practical estimate of things. The well known 'Four Methods' of Inductive Enquiry involve no appreciation of quantity; and therefore, as here maintained, they belong essentially to the popular attitude. In three of them we take no account whatever of anything but the complete presence or absence of the quality under consideration; and in the remaining one we merely consider somewhat vaguely whether there is more or less of the effect when more or less of the cause is present. Any help of this non-quantitative kind, it need hardly be pointed out again, carries us but a little way forward. Very few things or agencies are to be found, at any time or place, entirely present or entirely absent: and, when they are partially present, the guidance of our conduct in life will often seriously depend upon our being able to estimate exactly how much of each of them is present.

Almost any example will serve to show how urgently important are these quantitative considerations in order to supplement the results of the merely inductive or logical enquiry. Suppose the question were raised as to what is the cause why an iron girder of a bridge does not remain always of the same length, but is observed to expand and contract from time to time. This is an x for which we are asked to determine the A.

The usual Methods, duly employed, will serve well enough to ascertain the cause: that is, they will suffice to show us that it is not, say, rain and drought, but heat and cold which cause the expansion and contraction of the iron. So far good. But to the engineer of course it is all-important to be able to say with some accuracy how much expansion corresponds to any assigned increase of temperature. He has found by experience, let us suppose, that a tubular bridge in England requires a margin of 'play' of one inch in 200 feet: how much more must he allow for a similar bridge which he is going to construct in India? In cases such as these success or failure may depend upon an exact quantitative estimate.

Accurate measurement, in fact, forms the essence of true science. Our conclusions may be practically useful without this; but until we have 'made out the quantities' we have not completed our work. Indeed we might almost say that the extension of science from time to time is correspondent to the discovery of fresh measurable elements in nature; and that, within the limits of such extent at any given time, our progress is correspondent to the improvements made in the accuracy of measuring those elements.

The question may of course be raised whether considerations of this kind rightly belong to a treatise on Inductive Logic. The answer is that they lie just on the borders of our subject; in the sense that the leading principles of measurement ought to be discussed here, but that all details, and all account of the practical devices to be adopted to aid our efforts, are best relegated to the special physical sciences. We shall therefore steer between the entire omission of the subject, as by Mill, and its rather too practical discussion (at least for a logical treatise) as by Jevons, in his *Principles of Science.*

I. All measurement involves the conception of a *Standard* or *Unit.* These words are sometimes used synonymously; but there is a real distinction towards which they point, and to which it seems best to make them correspond. By a standard nothing more is meant than a fixed point of reference; a sort of typical specimen alongside of which other things may be brought and with which they may be compared. It forms the basis of measurement, but it does not carry us any further. The Unit is a standard which admits of being applied again and

again to the same thing so as actually to measure it. Take an example. The boiling point (due precautions being taken) is a fixed natural standard of the temperature of water. When another vessel boils we know we have the same temperature; and the possibility of recovering the same temperature is of immense use for many practical and scientific purposes. It furnishes one fixed point in the scale of temperature, but does not in any way, by itself, enable us to *measure* temperature. In a precisely similar way a sieve supplies a standard of magnitude. The largest stones that will pass through it will always be of the same size; but, beyond this, we cannot say how large any stone is by aid of a sieve. A foot-rule, on the other hand, is a true unit, for it is a standard which can be applied again and again to any particular length, so as to enable us to say *how* long it is.

For a rough description to start with, we may say that a Standard is nothing else than an arbitrarily selected specimen of the kind of thing which we want to measure, and that if it is one which admits of successive application in the way just above indicated, it becomes a Unit. Other things of the same kind may be *tested* by the Standard, but are measured by the Unit. That is, if any two of them are equal to the unit they are equal to each other; and if one of them be, say, ten times the size of the unit whilst the other is five times, then the former is twice as large as the latter. Or, if we do not want to compare two things together, but to obtain an absolute estimate of one of them alone, then we have attained as nearly to an absolute estimate as the nature of things will allow us to reach, by saying that the object in question is, say, seven times the unit. To take a concrete instance. We want to measure distance or length. The theoretic conception of the process is simply this. Some one with authority in such matters picks up any object, such as a stick, and says, Here is your unit of measurement: any material thing possessing length, say the trunk of a tree, along which this stick will go ten times is to be described as ten,—whatever it may please us to call the stick,—feet, cubits, sticks, or what not.

These remarks are only intended to indicate our starting point. The moment they are uttered a crowd of enquiries press upon us. Some of these involve points practical enough to

require the attention of even working mechanics; some are of a speculative-practical character, in that they raise the question, what would happen under circumstances which though they do not actually occur are nevertheless readily conceivable; whilst some are purely speculative, in that they are only concerned with contingencies which we are sure can never happen under any such laws of nature as now prevail. We must examine a number of the more important of these in detail.

(1) The first enquiry to suggest itself is one which arises out of the statement that the unit is a thing of the same kind as the magnitude we want to measure. What exactly is meant by this? We are reminded that a considerable process of abstraction is involved in the use of a measure of any kind. What we really want to determine is a quality, namely, an abstraction, for instance a *length.* Now length does not exist alone, but only as a quality of material objects, so that it is these which we always really estimate. This is necessarily the case, whether or not we are actually at the moment speaking of material objects. For instance, we may talk of measuring the mere distance,—as if only an abstraction were involved,—from London to Brighton: but what we actually measure is the road, that is, a portion of the earth's surface. Even when we speak of a distance estimated, 'as the crow flies'; though the problem is somewhat complicated, we shall find when we come to speak of indirect measurement that it is still something material, real or imagined, that we conceive ourselves to be measuring.

This process of abstraction of course greatly simplifies the number and variety of measures we require, for the extent of diversity through which we are enabled to identify a likeness of 'kind' between two things depends largely upon our powers of abstraction. A reference to the history of measures or units shows that primitive man was very slow in working his way towards the degree of simplicity we have already attained, though this itself is far short of what we hope to attain in time to come. There was indeed never a time, presumably, when it was considered necessary to measure wood objects by pieces of wood, and stone by stone. But the mere fact that even now, in England, feet and inches are used for measuring men and most familiar objects, 'hands' in measuring horses, poles and furlongs in measuring land, seems to show how slowly the conviction was

acquired that any one object which possesses a quality should serve to measure any other object which possesses the same. They applied their measures to the 'same kind' of objects in a much more restricted sense than does the modern scientific man who desires to see the same unit, for instance the metre, used by all people on the earth for the measurement of everything which possesses length, of whatever description the object may be.

(2) The next point of importance is raised by the statement that we 'apply' our measure to the object in hand, in order to determine how many times it is contained in that object. This statement must be understood under two serious qualifications; one of them practical only, the other of a more speculative character.

For instance it is obvious that only in rudimentary operations,—as when a carpenter measures a door,—do we actually put the foot-rule itself to the object we propose to measure. As our operations become more varied, and are carried out on a larger scale, our measurements tend more and more to assume an indirect character. To enter into this question here would involve a discussion upon the principles of mathematical procedure. It need therefore merely be pointed out that a very large part of the science of Mathematics,—nearly the whole, in fact, of Geometry as formerly conceived, and of much of its subsequent extensions,—is nothing else at bottom than a process of indirect measurement. For instance, costly expeditions have been sent out by most civilized governments to many different parts of the world, at the time of transits of Venus, and scores of calculators have been at work for years subsequently upon the results of the observations brought home by those expeditions. And all this has been done for the sole purpose of measuring a mere distance, namely, that from the earth to the sun. The outcome of the operation, that is, is a single measured distance, of essentially the same kind in respect of the mere facts, as when we make a direct measurement of so many miles along a turnpike road. For any theoretical purposes it is a mere accident which plan, the direct or indirect, we may have to adopt. From London to Dover we can measure directly; from Dover to Calais we must measure indirectly.

The various kinds of indirect measurement which we have

occasion to adopt are many, and the discussion of them in detail falls outside our present province. The briefest intimation therefore must suffice. Sometimes we do nothing more than employ an intermediate unit. Thus to measure a long garden walk with a foot-rule would be tedious and inaccurate. It is better to measure carefully a cord of, say, 100 feet, and then to use this enlarged unit; so that the primary unit is only indirectly applied. Sometimes the intermediate units, or what amount to such, are several in number. Thus in the simplest possible application of Trigonometry, say in determining a height, (base accessible), I may measure the length of a vertical stick, and two distances along the ground. I then have a simple sum in proportion, viz. that the desired height is to the length of the stick in the ratio of one of the two ground measurements to the other. The three directly determined magnitudes thus become indirect units for the measurement of the fourth. In the same way we may proceed onwards, introducing more and more complicated calculations, until we have exhausted all the resources of Trigonometry and all those of the higher mathematics which this may invoke.

When therefore we speak of 'applying' our unit to the object to be measured, it must be remembered that the greater portion of the work is generally done by indirect means, namely, by calculation. But this does not alter the essential nature of the operation: in fact it is only the intermediate part of the process that is, so to say, done on paper or symbolically rather than by actual work of hand. We start always from a base line, or something equivalent, which has been actually measured. And we end always with some magnitude which bears the same sort of relation to that with which we started that the carpenter's plank does to his foot-rule. And what we thus end with can always be expressed as a magnitude to which we might hypothetically apply our unit. When, for instance, we give the distance of the sun from the earth as 93,500,000 miles we can always interpret this by saying that what we mean is that a certain number of yard measures laid end to end would stretch from one to the other.

The above qualification is of a practical character, and implies at most a somewhat circuitous path to our end. The next is of a rather more serious character from a speculative point

of view. What is meant by applying our unit,—that is, the same unit,—a certain number of times to the object before us? In such examples as those above, in which we were concerned with the quality of *length*, we could take the phrase in the most literal sense. We can if we please put the same identical rule again and again against the plank, just as the Hindoo devotee can lay his own body down upon the ground and measure the road from Delhi to Benares. But in other cases some explanation and assumption of equivalents is required. In *weighing*, for instance, we cannot put an iron pound into the scales repeatedly, and mark each time where its weight left off so as to begin at the right spot next time[1]. We take a number of different weights, and proceed upon the assumption that any number of these which balance each other may be combined into a total without mutual disturbance, on the same principle that any number of yard measures, which equal each other, when laid end to end will come to the same result as if the *same* measure were applied a number of times successively. The assumption is justified in each case; in the latter on geometrical grounds and in the former on physical grounds, as to the nature of which we cannot here enquire.

When we come to some other kinds of measurement this particular difficulty asserts itself in a more prominent manner. How do we employ our unit of *time*, for instance? It has been sometimes attempted to draw too sharp a distinction between this case and that of space, on the ground that we cannot take a portion of time and apply it to the event to be measured. True; nor can we take a portion of space, or a portion of weight. What we do is to take a piece of matter which we suppose to retain unaltered the same quality of length or of weight, and we apply this, or its precise equivalents, as often as need be. So with time. What we do here is to take an event,—a piece of event, we might call it, as we called the other a piece of

[1] We cannot do this with the scales, because the pan will not remain at the point to which the first application of the weight depressed it. But it is worth remarking that indirectly, through the momentum, we can in certain cases really keep on applying the same weight, as we apply the same foot-rule. In driving in a pile (resistance being uniform) fifty equal applications of the falling block, each commencing just where the previous one left off, will produce precisely the same effect as one block fifty times as heavy and falling the same distance.

matter,—and we apply this. The essentials of the operation seem to be the same in each case.

The difficulty is therefore of the same kind, and the same sort of assumption is required. Since we cannot appeal to the same identical unit repeatedly, as in length measurement, we must find a number of exactly equivalent units, as in weight measurement. In other words we must discover a number of events all of which possess exactly the same time-quality of duration. As every one knows, the device we employ for this purpose, at any rate where short durations are concerned, is that of the successive beats or swings of a pendulum. And to save the trouble of counting these we employ a machine called a clock, which registers and indicates the number for us. But what is our guarantee that these distinct units are really of the same duration? Has our certainty here the same warrant as in cases where distinct foot-rules or distinct weights are concerned? This question demands closer enquiry.

II. The fact is that there is one general assumption underlying every employment of a unit or standard. It may be described, in a general way, as consisting in our reliance upon the Uniformity of Nature; not in the causative or sequence sense of that many-sided expression, but in that of continuity or permanence. Something was said about the nature of this distinction in a former chapter, but it deserves some further consideration in the present application.

(1) Start with a purely practical example. It is obvious that the utility of a carpenter's foot-rule depends upon the fact that the wood of which it is made does not continue permanently to shrink, and does not vary in magnitude from time to time. It does shrink a little when the wood is quite fresh, and it varies a little with the temperature ever afterwards, but it does not vary in any degree which matters for the carpenter or for those who employ him. But when we come to finer work this consideration is of importance. For instance, there is a serious difficulty in the way of accurately graduating our thermometers, in the fact that the glass tube, (and consequently the scale marked upon it,) continues slightly and slowly to shrink for a number of years after the instrument is made. So, again, the difficulty of retaining a fixed length in our measuring rods when we want to survey very accurately in

a hot climate. Mr Piazzi Smyth, in his quaint work on the great pyramid, considered that one of the qualifications entitling the sarcophagus to play the part of an eternal measure of volume rested on the fact that the porphyry of which it was made, unlike the glass of our thermometer tubes, had probably had millions of years to finish its process of contraction[1].

(2) These practical difficulties can be met by practical expedients, the amount of time and trouble demanded for this purpose being dependent upon the degree of accuracy required for the operation in hand. But turn now to what may be called, in contradistinction, the speculative-practical difficulties: those, namely, which we can readily conceive as presenting themselves though we feel sure that they never will actually trouble us. We live in a world in which we find ourselves surrounded by plenty of rigid matter which does not seem to vary perceptibly either in size or in shape. But we can conceive it otherwise; and, were it otherwise, it is not easy to see how we should ever acquire any such thing as a unit of length or any other unit which involves this. Suppose our experience were such as would be presented to a colony of jelly-fish which lived on the surface of the water in the midst of an ocean. So far as they and the water and most of the sea-weeds were concerned, they would hardly find a material at hand which would furnish them with the notion of any such thing as a standard of size or shape. *Mass* they might understand from the experience of their own bodies, but in the entire absence of anything rigid they could scarcely realize what was meant by a linear standard or anything which depended on this. Anything to call Science would be out of their reach, for they would have nothing to provide practice in measurement, or even to furnish the notion of measurement quantitatively.

It is here, then, that we are reminded of the necessity of

[1] It is notoriously difficult to get at the first version of any suggestion or theory; but I confess I was surprised at the following passage—"C'est ainsi que Mr Greave, mathématicien anglois, a voulu se servir des Pyramides d'Egypte, qui ont duré assez et dureront apparemment encore quelque temps pour conserver nos mesures, en marquant à la postérité les propositions [? proportions] qu'elles ont à certaines longueurs dessinées dans une de ces Pyramides" (Leibnitz, *Ed. Erdmann*, p. 239:—he refers to the *Pyramidographia* of John Greaves, Savilian professor of Astronomy at Oxford, published in 1646; where see note to p. 94).

Uniformity of Nature, not in the sense of Causation, but in that of Persistency,—and persistency in details of physical structure,—if we are to be able to measure or infer with quantitative accuracy. The mere indestructibility of mass or matter, remember, is not enough. On the common molecular theory, and still more on the Boscovichian theory of mere mathematical points as centres of force, there is no absolute contradiction involved in the supposition that all substances should be in a constant state of flux or vibration, not only in respect of their shape but in respect of their size also.

The assumption in question underlies every selection of our units. It might seem as if a unit of weight, at any rate, or rather, to keep to the strict truth, a unit of mass, could be secured so long as we regard matter as indestructible. But this is really not so. What we have to employ are sensible portions of matter, and everything turns upon the assurance that these do not vary in amount. The particles of matter must not only not vanish, but they must adhere continuously together: that is, the standard must not in any degree dissolve or corrode. We feel sure that this condition is fairly well secured in respect of gold and platinum, and perhaps of some precious stones, &c.; but it is by no means sure that we could continue to trust to the invariability even of these for millions of years if we decided to look so far ahead[1].

(3) These suggestions admit of being pushed further, into the region of the purely speculative. We remarked before that if the carpenter's rule were liable to change he could not properly measure the door. But suppose the door also was

[1] Professors Thomson and Tait have thrown out the suggestion (*Elements of Nat. Phil.* p. 119) that, owing to the demonstrable secular alteration in the velocity of rotation of the earth, "the ultimate standard of accurate chronometry must, if the human race live on the earth for a few million years, be founded on the physical properties of some body of more constant character than the earth: for instance a carefully-arranged metallic spring hermetically sealed in an exhausted glass vessel". Surely this is a rather sanguine view of the permanence of the properties of matter. One would not have supposed that anything which had had its shape produced by melting, drawing, or hammering, could ever afterwards be thus unchangeable. And how about its unaltered temperature? And are we certain that a solid does not 'evaporate'? But probably the suggestion is playful. (It may be remarked that the latest theories as to the nature and constitution of molecules and atoms do very strongly suggest that solids evaporate.)

changed at the same time, and in the same proportion, as well as everything that had to pass through it; would any harm ensue to any one, or would any of us be able to perceive that a change had taken place at all?

This question cannot be answered off hand. If we are to regard it as a merely geometrical one,—that is, if we do not introduce any physical principles such as those of dynamics, —I apprehend that no change whatever could be said to result. In fact we might go further, and say, that since mere magnitude is purely relative, it is not easy to attach any significance whatever to a supposed simultaneous and proportional change in the magnitude of all things. This seems to me, in fact, like an analogous suggestion sometimes made in Political Economy, when it is asked what would follow from a similar change in all values. 'Cost of Production' *is* absolute, so far as Political Economy is concerned; but 'value' is purely relative, being nothing else than Comparative rate of Exchange. However plentiful or scarce things might be, yet if they continued to exchange against each other as before, no change of value whatever is introduced. So with the mere space attribute of magnitude. If all our measures were halved, simultaneously with all the persons who had to use them and all the objects to which they had to be applied, no perceptible change of dimensions whatever would have been introduced.

It is when we introduce physical considerations that the real complication arises. Suppose that everything simply contracted to half its present size lineally, no other alteration being contemplated. Direct measurement, as we have just said, would detect no change. But, other things remaining the same, the world being reduced in size would immediately begin to rotate faster. If it be asked, how we could ever find this out, since all the other moving objects would also have their velocities altered, the answer is that according to known existent laws these velocities would not all be altered in the same proportion. Pendulums would vibrate at a different rate, falling objects would move at a different rate, so would the various heavenly bodies; but these various rates would no longer all preserve their old proportions. We should immediately begin to find that something was wrong.

Of course this would not furnish any absolute proof as

to what had happened. Assuming the existence of such a dislocation of nature, those who found themselves in the midst of it would have the option of more than one interpretation. It might be that, not the magnitude of objects, but rather the laws of their behaviour had been changed; and possibly this would seem the most plausible explanation under the circumstances, so far as we can attempt to balance probabilities in such a case. At any rate this would be the probable explanation if such a shrinkage were small and gradual. That is, the faint discrepancy observed would make us begin to doubt the absolute truth of the familiar Laws of Motion.

III. The next point to notice is what may be called the *arbitrary* character of our units. We shall have something to say presently about their historic origin, when we come to consider more in detail the characteristics of three or four of the most important units. But so far as the actual existence of the fundamental units is concerned, we shall best understand their nature by conceiving that some one were granted authority to select whatever objects he chose, and to pronounce that these shall be our standards. He might find them in himself if he pleased. He might say, for instance, that the length of his foot or of his forearm shall be our lineal unit; his weight our standard of weight; the rate at which he walks our standard of velocity; and the interval between the beats of his heart the standard of time. If he decided thus, he would, for various reasons, not have made a very good choice; but as against the mere capriciousness of his selection no complaint could be raised. Our present foot and pound have little better warrant than these would have, so far as the nature of things is concerned.

The defect of such proposed standards would be found in the inconvenience of employing them, the difficulty of accurately determining them, and the certainty of sooner or later losing them. Accordingly we prefer material objects of as durable a character as possible, but the selection of them is still theoretically a matter of caprice, for it depends upon an act of the legislature. The pound and the yard are actual things: they can be visited at their proper receptacles: and they are described by Acts of Parliament. The 'second', our unit of time, stands on a slightly different footing; for it is an event, or rather a fraction of an event. It has not been defined

by an Act of Parliament and it is not kept in any public repository. The ultimate original to which we appeal is a sidereal revolution of the earth, and therefore does not stand in need of safe custody anywhere. But the selection of this standard is really as arbitrary as is that of an assigned lump of metal.

We spoke, just above, of *fundamental* units. The contrast between these and such as are derivative is an important one. The logical or primary conception of a unit, in contrast with the practical substitutes which we may employ, is, as has been said already, that of something of the *same kind* as that which we measure. We are always obliged to compare concrete things, but we compare them in respect of the same quality. Thus we measure length by a unit of length; that is, we compare the foot-rule and the plank in respect of this attribute of length. We measure mass by mass, or, to speak more vulgarly, weight by weight; that is, we compare the standard pound with a quantity of sugar or tea, in respect of their attribute of mass or weight. In the same way we should logically proceed to compare velocity with velocity. We might take the velocity of a walking pack-horse as our unit and compare that of a coach with this standard.

If it be enquired how we could compare different velocities in this way, the answer can (theoretically) be given easily enough. We should do it in the same way as we do it with the weights, that is, we should compound the velocities. Let the pack-horse, walking at its accustomed pace, draw a barge (say), and along that barge let another horse walk at the same pace. If a coach on the bank of the canal just keeps pace with this second horse its velocity would be double the unit; that is, we should represent it by 2. There is no more intrinsic difficulty in compounding and multiplying velocities than in doing the same with weights; and we only require the same fundamental assumption as to the mutual non-interference of the compounded elements.

In practice, as need not be said, there are insuperable difficulties in the way of conveniently compounding, and therefore of measuring, different velocities directly. Accordingly though primitive people, and indeed all of us in our non-scientific attitude, have a number of popular *standards* of velocity we

have nothing to call a *unit* of this direct kind. Thus we might readily compare the speed of some moving object by saying it was that of "a walking pace", or, more loosely, that something was moving as "swift as an arrow". We should have to select a different object for the comparison in each case; and this limitation is the essential point about a mere standard.

This leads us to the important distinction between what are called fundamental and derived units. The magnitudes with which we deal are variously interconnected, and hence it becomes possible to measure some of them by means of others. The choice of one or another of them for this purpose depends entirely upon convenience, the principal factor in this convenience consisting in the practical possibilities of accurate determination. Thus velocity, as we have seen, is an inconvenient unit to employ directly. Accordingly we 'derive' the unit here from those of space and time, which admit of extremely accurate determination, by saying that the unit of velocity shall be,—not an assigned concrete object moving at its accustomed pace, but,—an object which moves over a unit of space in a unit of time.

The three fundamental units, as now almost universally recognized in science, are those of length, time, and mass; since these fulfil to the highest degree the requisites of accurate determination. It would be beyond the province of this work to enter into further detail about these; and what little more has to be said about them is best reserved for a few pages. But the reader must understand that the choice of these, as the fundamental elements, turns almost entirely upon the convenience above indicated[1]. Thus instead of deriving the unit of velocity from those of length and time, we might have derived that of length from those of velocity and time. Thus we might have selected for our purpose, respectively, one second, and the velocity of a body which keeping on the same selected parallel of latitude goes round the world in one sidereal

[1] As Professor Everett points out, we might have taken only one of these particular three (namely mass); and have selected, a definite mass, a definite amount of energy, and a definite density. When these were determined we could deduce from them the units of length and of time. (*Units and Physical Constants*, Ch. II.)

day. The unit of length would then be the distance which such a body passed over in one second. If the latitude of London were selected this length would be, roughly, about 300 yards.

It is worth noting that though the existent order of derivation, namely that of making velocity the secondary unit, is the only one now adopted anywhere for scientific purposes, there is considerable historic support in favour of the exactly opposite order, namely that of starting with a certain concrete velocity actually given to us in experience, and deriving the standard of distance from this and the time unit. Thus long distances used to be measured by so many 'days' journeys'. The pace of a camel was quite sufficiently regular and familiar to serve as a unit of velocity, and the distance gone over in a day (the unit of time), furnished obviously the unit of distance. Thence the whole distance, say from Damascus to Nineveh, was given in terms of this derived unit. Similarly in mountain countries to this day. The peasant or the guide will give the distance in '*stunden*' or hours, since the pace of a mule, or of a man who has no object in walking against time, is sufficiently uniform to furnish a standard. That what is being thus measured is really a distance, in terms of time, seems clear; for if we expressly ask a peasant whether it will take us so many 'stunden' to do the distance, which he has the moment before declared to be two *stunden*, he may say, No; only one and a half.

IV. A few remarks may be made now about some of the main characteristics of the principal units in use. Three of these, as above remarked, may be called fundamental in the sense that the others are derived from them. These three, therefore, namely those of length, of time, and of mass, are the only units of the purely arbitrary or *sample* class; that is, the only ones as to which we can give no other account than by saying; This which you see before you has been authoritatively declared to be the unit, and it needs no appeal to other units to determine it.

(1) Units of length. We have already said nearly all that is required for our present purpose about these. Originally an immense number of different units were in use in every country; and those in the same country were not only entirely independent of each other, as applied to length, surface, and

volume, but also as applied to the various kinds of commodities which had to be measured. Their origin was also very various. The smaller ones were, in all probability, as need not be said, derived from such natural magnitudes as every one carries about with him,—e.g. the foot; the cubit, or half arm from the elbow to the finger tips; the yard, or whole arm; the thumb width, or inch; &c.,—or from such natural processes as will readily furnish a magnitude; e.g. the pace. The longer measures were derived from results which under actual conditions were sufficiently uniform to convey the required accuracy of information, e.g. a furlong, a bow-shot, &c.

There is now a general agreement to use as far as possible but one such standard, and to use it all over the civilized world. Thus scientific men almost everywhere have come to adopt the *metre*[1]. This was always a fundamental unit in the sense used above, for it was not derived from any other units of different kinds; and even the looser sense in which it could be called 'derived', namely in the sense that it was not determined by an actual material standard but calculated as a fraction of a quadrant of the earth's surface, has now been abandoned. The actual metre *is* a certain platinum rod kept in a certain receptacle in Paris.

The possibility of trusting in this way to *sample* standards rests upon the fact that there are substances,—such as platinum,—which are rigid, and for all practical purposes incorrodible. Had the only known metals been as perishable as iron, we should have been forced to adopt some such plan as the original scheme of the French, and to have taken the earth itself as the primary standard, and a fraction of one dimension of this as the practical working standard, if we legislated for many centuries ahead.

(2) Units of Mass. The main difficulty here is that which is felt by those who have had no kind of mathematical or physical training, in clearly distinguishing between mass and weight. What the State fixes by its standard is a certain mass, that is a certain fixed bulk of matter, and it remains a 'pound' whether it be taken to the top of a mountain or to

[1] Modern physical science finds it often convenient, for reasons we cannot here enter on, to select the *centimetre*: the so-called C.G.S. system adopts this, the gramme, and the second, as units of length, mass, and time.

the bottom of a well, or be removed to a different latitude. But the point of effective interest to us in most cases is, of course, the *weight* of an object, that is, the force produced by gravity acting on the mass, and this varies according to the latitude and the height above the surface of the earth. The ground of the common confusion depends upon the fact that this variation, though of the utmost scientific importance, and perceptible by sufficiently accurate instruments, is too slight to attract ordinary notice. The easiest way of realizing the notion of mass is to conceive oneself slowly pushing a heavy body on the water, say a barge. A certain effort is required to start it with a given velocity, and the same effort is required to stop it again, and this though we do not lift it from the water so as to feel its weight. A massive wheel or grindstone when set in rapid rotation, or when stopped, offers the same experience.

The reason why in practice we seldom think of mass except through gravity, that is, as exerting downward pressure, depends like most other things upon the conditions of our ordinary life in the world. Weight is practically important on two main grounds: partly because, owing to friction and to differences of level, we incessantly have to *lift* heavy bodies; and partly because owing to the solidity of so many substances we often find it easiest to measure volume or bulk indirectly by weight. If most commodities were liquid we should measure them by volume instead of by weight; and if most surfaces were smooth we should generally push an object instead of lifting it; and in such a case (especially if the variations of level as between one place and another were less than they now are) we should be familiar enough with the conception of mass through its fundamental attribute of inertia.

(3) Units of Time. In selecting units of length we said that primitive people had displayed an almost bewildering variety of choice: the only approach to what can be called natural standards being found in the various parts of the human body, e.g. the arm and the foot: and that therefore the essential arbitrariness of that choice is soon forced upon our notice. In the case of *time* it is otherwise. Here we find three natural standards so obviously set before us that the only question which primitive man had to confront was how to make a choice

between them, and the principal later difficulty arose when men had to decide how these standards were to be brought into relation with each other. The concrete objects which supply these measures are of course the sun and the moon, and the well-marked events in their career which furnish the units are the periodic changes known as years, months (lunar), and days. Each of these has been universally employed for this purpose from immemorial antiquity, and practically no other standards than these ever have been employed.

In taking a theoretical view of the subject it is well to pause a moment in order to consider what might have happened under two different assumptions, which we are at liberty to make, as to the course of nature. In the first place, what would have happened if these three measures had exactly fitted in with each other as do the minute and hour hands of a watch? A prodigious amount of trouble would clearly have been saved to early calendar makers; and doubtless historians and others who have to deal with the far past would have had their labours rendered much simpler and more accurate. Men would not have had to wait for a Gregory to reform their Calendar. But what about the speculative results of such a hypothesis? The absolute agreement of two such powerful timekeepers might have rendered it for long impossible to realize the notion of the relativity of time as we now conceive it. Of course we should eventually have attained our present attitude towards the problem if we had succeeded in giving a physical explanation of the agreement amongst these heavenly bodies, in the same way as we now account for the exact agreement of the two motions,—namely those of translation and rotation,—of the moon. It is however really a doubtful question whether the fact of such precise agreement in respect of measures so deeply affecting mankind would not itself have been a great hindrance in the way of our ever realizing the problem aright. But, in the face of what was said in a former chapter about the nature of admissible suppositions, it would be necessary to go into somewhat minute detail if we wished to follow up this speculation to any profitable purpose.

Again,—and here we are on clearer ground,—what would our position have been if we had possessed no such conspicuous

and therefore universal timekeepers as the sun and the moon? We can readily conceive such a state of things, for it would have resulted had the periodic times been very much longer, or had these luminaries been persistently clouded over; or it might have been brought about in other ways. Do we then carry about with us, or can we easily lay our hands on, any such convenient and suitable units of time as are furnished, in the case of length, by the foot and the arm? It seems to me that we have such,—at any rate for very short intervals of time,—in the beating of our hearts. Had this been much louder and a little more regular, so that others might hear it and find that it closely corresponded with their own, it is quite within the bounds of possibility that it might have been commonly employed to estimate small intervals[1].

As soon as the two great natural timekeepers,—the sun and the moon,—were found not to be in exact agreement commensurably with each other, a choice had to be made between them. We know, historically, what an immense amount of labour had to be gone through before this choice was made. In fact it was not until the genius of Newton had placed the laws of motion upon a sound basis that it was possible to make a really scientific selection. We now know, if we may express ourselves in pre-Copernican language, that it is neither the sun nor the moon but the fixed stars whose motion is regular. The nearest approach we can make to an absolute measure of time is attained by selecting the sidereal day, that is, the interval between two successive passages of the meridian by the same fixed star. The solar day is very nearly what we require, but not exactly. The sidereal day, which is about 4 minutes shorter, suits our purpose exactly. This is only another way of saying that the velocity of rotation of the earth is invariable, and that the so-called fixed stars are at such an enormous distance from us that any relative motion which they display amongst themselves may be neglected.

Physicists, of course, know well that neither of these state-

[1] I have myself boiled an egg in this way, with fair success, in the absence of a watch; and it is quite possible that an unconscious appeal to this timekeeper may count for one factor, even under existent circumstances, when we have to determine short intervals of time.

ments is rigidly true. There are tolerably conclusive reasons to prove that the velocity of rotation of the earth is very slowly but continuously diminishing; and, as regards the fixity of the fixed stars, a 'proper motion' has been detected in so many of them, and this in such various directions, that there can be little doubt that in lapse of time their 'drift' may be so large and so irregular that they would no longer serve the purpose of an exact indication as to the true time of transit.

These three units,—those of length, mass, and time,—are the fundamental units of physics. In reference to each alike the question may be raised, how we know that it really remains invariable: and the same reply must be given in each case; to the following effect. We must admit that as regards the direct testimony of our senses we have no guarantee, or almost none. If we were to depend upon our powers of deciding, merely from the sensations excited in us, whether some proposed standard has actually varied, this would not only presuppose invariability in those powers of sensation themselves, but we should find ourselves limited by their degree of accuracy. If I have handled a weight of a kilogramme on one occasion, I can, say a year afterwards, judge rudely merely from recalling the former feeling of strain in lifting it, whether a certain weight in my hand is or is not as heavy as a kilogramme. And similarly with our recollections of lengths of space and intervals of time. But such reliance as this is far too rude and precarious to be worth taking account of. If the question asked, however, be as to the grounds of the practical security we feel over the range of our ordinary experience, the answer must be that the only test here demanded is that of permanence of relation, and that past experience gives us abundant security for this. We are perpetually testing our measures against other objects, and comparing these objects against each other, and we find the results in harmony. But if the question be pushed still further, and it be demanded what security we have that the magnitudes, say, or the masses, or the rates of movement of all material things, have not undergone a simultaneous and proportional change, we must give a different answer. We can only say that an enormous and ever-in-

creasing number of facts can be reduced to law upon the assumption that there is no such change; but not, so far as we know, or as any one has taken the trouble to try to ascertain, upon any other assumption. As Clerk-Maxwell has said about a somewhat similar suggestion (*Matter and Motion*, p. 36) such a supposition "is in contradiction to the only system of consistent doctrine about space and time which the human mind has been able to form."

CHAPTER XIX.

STANDARDS AND UNITS.

(2) Psychical.

I. In the last chapter we discussed the general characteristics of a physical standard or unit: we must now proceed to enquire whether the same conceptions can be introduced into the region of psychical phenomena. There are two slightly divergent links of connexion, by which we seem to be led on from one of these orders of investigation to the other.

(1) In the first place it must be remarked that however strictly we may intend, when engaged on physical measurement, to exclude all purely psychical estimate, we find ourselves quite unable to do so. Some actual estimate of a quantitative kind concerning our own sensations will be found to be involved in almost every case, even though we may profess to be engaged on an entirely objective measurement. Carry on the external or mechanical processes as far as we can, we shall find that at some stage or other there is "a bit over", which, if taken into account at all, has to be directly estimated by an appeal to our feelings, that is, without any objective aid.

Begin with one of the simplest cases. I am shown a door, and am asked how high it is. If nothing more than a very rough estimate were required I should merely look at it and say that it measures about seven feet. If a little more accuracy were desired I should apply a rule, and pronounce the door to be (say) seven feet three inches; this last addition being very probably nothing more than a mere estimate. If it were necessary to be still more accurate I should measure the inches also, and confine the resulting estimate to the eighths of an inch. And if finally the object were one in which the last refinement of accuracy was demanded, as in astronomical operations, we might employ a vernier to subdivide to minuter

fractions of an inch; but even then, the decision as to which line on the vernier lay nearest to the line which marked our boundary would still have to be made by what we must term a direct subjective estimate of magnitude.

Similar considerations are involved in the measurement of time. If after listening to a sermon I were asked how long it lasted, I might give a rough estimate by saying that it was about half-an-hour long. And if greater accuracy were needed, and there were a clock at hand,—one which we could hear but not see,—I might say that the sermon began just as the clock struck twelve, and ended just about three minutes after the half-hour. The process here, it will been seen, is of much the same kind as that with the foot-rule: mechanical aids as far as possible, or rather as far as desirable, and then an estimate, and an estimate, remember, of the very kind of element which we are undertaking to measure. When very great refinement is required, as in astronomy, we make use of a clock which accurately ticks seconds, and the fraction of a second which remains over is estimated just as the mechanician estimates that of an inch.

We might adopt the same plan in the case of mass or weight; and in fact we do so judge occasionally, and to a certain extent, in this department. For instance, if I wanted to know whether a certain parcel weighed more than ten pounds, I should start very likely with a direct but crude estimate. If I felt doubtful, the parcel would be put in the scales; but suppose that no other weight were at hand than a single one of 10 lb., and that the parcel outweighed this, I should probably do very much what the carpenter does when measuring the door. That is, I should try to reckon how much was over, by a direct estimate; for instance by pressing my finger on the lighter scale, and judging that the requisite surplus pressure was about, say, two ounces.

One reason why this conception of the nature of measurement is not more familiar to us consists in the fact that there are so many ways of transferring the appeal from one sense to another, that we seldom practically carry out the process with the successive steps of refinement indicated above. For instance, if a workman had to decide the relative length of two short metal rods he would start, no doubt, with an eye-estimate

but if he wanted to be accurate he would employ a gauge. That is, he would transfer the appeal to the sense of touch, by feeling whether one of the rods had any 'play', and if so how much, within a gauge which precisely fitted the other. In measuring a weight such a transfer, from one kind of appeal to another, is always made very soon. The chemist does not attempt to make his decision by any feeling of difference of muscular sensation; he judges by the eye, by noticing whereabouts on the scale the index of his balance is hovering. So with all accurate measurements of time. Wherever it is possible we employ some sort of chronograph, and these instruments generally rest, in the last resort, on an eye-estimate of linear magnitude. By a mechanism, of rotating wheels or falling bodies, the intervals of time are actually marked visibly on a scale, and then this scale is measured just as we should measure any other linear magnitude. Those who are interested in mechanical devices will find the description of some of the more refined of these instruments a fascinating subject of study[1], but they clearly do not belong to our province of enquiry.

It seems therefore that, whether we wish it or not, we cannot avoid the process of directly estimating our sensations in a quantitative way, that is, of measuring them. Accordingly the conception of standards and units in this department, with all their attendant difficulties, is necessarily forced upon our notice. But there is another indirect reason of a somewhat paradoxical character, which tells in the same direction. The very fact that physical science tends to reject and depreciate one after another of the familiar appeals to our sensibility, upon which at one time our measurements of magnitude had mainly depended, causes a gradually increasing accumulation of subject-matter for some neighbouring department. In other words, what the Physicists tend more and more to let alone, it becomes

[1] It is in modern gunnery experiments that some of the most minute time measurements are needed. Not only is it necessary to know the precise velocity with which the shot issues from the muzzle, so as to compare one gun with another; but it is also desirable to know at what rate this velocity is acquired within the gun itself, in order to calculate the strain at different points of the tube. The authorities claim to measure to the millionth of a second. (For a full account of two sorts of chronograph used at Woolwich, see the Official *Treatise on the Construction of Ordnance.*)

more and more the business of the Psycho-physicists to take in hand. And as the conception of a unit of the latter kind belongs as much to the province of Inductive Logic as does one of the former, we are equally bound to notice it here.

(2) It has been already indicated that in many cases, as science progresses, we cease altogether to measure, even by any method of transferred appeal, the same element as that which we started by regarding as the most important. We no longer measure the same thing in a different way, but we measure a different thing altogether. Consider for instance what we mean by *temperature*. The root idea of this, and probably nearly all its significance still to many persons, consists in a certain feeling, namely that of warmth or cold. And this is what we start at first with attempting to measure. When we say that one day is hotter than another, or that the difference between the temperature of day and night is less in England than in Egypt, what we are mainly thinking of is our own sensations and those of persons like ourselves. This element admits of direct measurement, to a certain rude extent, and is indeed still so determined for certain purposes:—as for instance by old-fashioned nurses and bath attendants, who commonly judge of the temperature by thrusting the hand or elbow into the water. For all but the rudest purposes however we now judge the temperature by help of a thermometer. The ultimate test therefore is that of the eye. It is an eye-estimate of linear magnitude, namely the length of a column of mercury in a glass tube. The change in the nature of our appeal here is obvious; but the really important point for the purpose now in hand lies in the fact that there is not merely a transfer of appeal from one order of sensation to another, but something much more than this. We are no longer proposing to measure the same thing. What we now have to deal with is not a sensation but a physical property.

The reasons for this change of procedure seem obvious enough. They arise out of that steady progress of science which by detecting new uniformities is continually diminishing the relative importance of our mere sensations as compared with the underlying properties of things which are the causes of them. It is not, of course, that sensation in general can ever lose any of its importance to sentient beings,—it is, and

ever must be, the source of pleasure and of pain, and involve with this the issues of life and of death,—but as our knowledge of nature advances we begin to find that what we call physical qualities or agencies assume preponderating importance, because they are indirectly a sure index to so many different sensations.

The reader will see what is meant by this if we revert to the instance of temperature. Originally the mere feeling must have been almost everything, and when the primitive man talked of heat he would naturally be thinking of little else than this. But when we now speak of heat we think,—at any rate the scientific man thinks,—of an underlying quality or agency or state which shows itself in a vast variety of ways. Heat means that which expands most solids, which vaporizes liquids, which softens some bodies and hardens others, which produces an immediate effect on vegetation and a remote effect on public health. All these contingencies are of importance to us, no doubt, because we are sentient beings, but the extension of knowledge thus produced tends to break down the almost exclusive connection which once existed between heat and *one* class of sensations. Hence it comes to pass that the chemist or other physicist who is engaged in studying heat gives comparatively little attention to the special sensation; it is indeed quite probable that he may carry on a long course of experiments without once thinking whether any part of his body feels warmer or colder in consequence.

The same tendency shows itself in many other directions though we cannot perhaps find any other example quite so distinctive as this. For instance, it does not seem to me to be very far-fetched to suppose that the root-idea of *distance*,—that is, of great lengths which we have to walk over, as contrasted with short lengths which we handle,—may once have been *fatigue*. Whether this be so or not there can be little doubt that the relative importance of this element must once have been much greater than it now is. In a very primitive state of society the effective difference between a mountain pass of 5000 feet and one of 2000 feet would largely consist in the extra fatigue involved in going over the former; the importance of the difference as reckoned merely in *time*, which the trader naturally thinks of, being probably a later growth. If this sensation ever really did play such a predominant part in the

conception of distance, as we know that the sensation of warmth did formerly in that of temperature, we should naturally have begun by directly estimating this sensation; and we might have estimated the objects themselves,—the mountains or passes,—by the amount of this sensation which they produced in us.

The above is mere speculation, but it will serve to illustrate an undeniable tendency. The height of a mountain pass means of course, even to savages, a great deal more than the attendant fatigue, just as to ordinary persons at the present day temperature means a good deal more than the attendant feelings of warmth or cold. And this change of estimate appears to be universal. Wherever we turn in science we find that there is a tendency to diminish the relative importance,—so far as anything to do with measurement is concerned,—of any one class of sensations, however important or intense these may be in themselves. It is the underlying quality or attribute,—what we regard as objective,—that we seek to measure. This alone seems to come up to the desired standard of precision; this can admit of accurate intercomparison, not only amongst different people at the same time, but over long periods of time. Accordingly we devote our attention to measuring this element, and we seek to deduce what the sensations will be as far as possible from this.

In some cases, though the quality or agency which we thus measure has of course a basis in sensation, this aspect of it had never succeeded in attracting popular attention. The phenomenon may have been of very rare occurrence and have demanded some artificial effort for its production, or it may have been of very little use or significance in popular experience. Thus Electricity might be popularly estimated by the strength of the shock which is felt, just as temperature is popularly estimated by the degree of warmth of which we are conscious; and it doubtless would have been thus measured long ago if it had been a phenomenon of such frequent and obvious occurrence as to play a prominent part in our ordinary life. As things are, however, the consideration of this agency has remained almost entirely in the hands of scientific persons. Hence it is this agency itself, and not the resultant feelings, which have been studied; and inasmuch as the former, unlike

the latter, does not readily lend itself to even a rude degree of measurement, this quantitative aspect of it was for long neglected. At the present time, we need not say, the case is very different. Numerous and careful attempts have been made to ascertain and define the best units for the purpose; so that the electrician is now able to state numerically the strength and the quantity of the agency in any given current, as well as a number of other characteristics which it presents or encounters but which are far too technical to be mentioned here.

II. As a consequence of all this, our mere sensations were coming,—absolutely, to a considerable extent, and relatively to a very great extent,—to be left out of account in respect of measurement. Quite recently however systematic and laborious attempts have been made to bring these simple elements of our experience under quantitative estimate. The merit of first opening out this new and interesting line of enquiry is prominently and almost exclusively due to Weber and to Fechner.

In order to understand what exactly it is which we are aiming at in this direction, and what are the difficulties in our way, we will begin with the detailed discussion of an example. Take then a form of sensation with which we are all familiar, selecting one in which the mere sense element, in contradistinction to the intellectual element,—that which we feel, as distinguished from that which we infer,—is at a maximum: namely some kind of pain. Suppose we select toothache. No one hesitates to use some of the language appropriate to measurement when speaking of this feeling. We can say of it, at any given moment, that it is about as bad as it was on some former occasion: that it is better or worse: that it is very much better or worse, and so on. All these expressions, so far as they go, point to some form of quantitative estimate.

Now appeal for comparison to that very instructive example of *temperature*, in which, as we have seen, so much progress has been made in the way of transferring our appeal from the subjective to the objective; from the feeling to the agency which underlies it. Why do we not seek to make the same transfer in the case of the toothache? For several reasons. It is not, remember, because the basis of such transfer is entirely lacking. The sensation of toothache depends immediately

upon a physical condition of the tooth, just as the feeling of warmth in the finger depends more remotely upon the physical condition of the water in which I dip it. But in the former case the dependence is still very obscure, and is determined by so many conditions, that a description of the physical condition, in terms of magnitude, would not even yield that approximate index to the resultant sensation which is afforded by saying of the water that its temperature is 120° Fahrenheit. Then again the sensation by its intrinsic force is apt to overpower all other considerations. The physical condition of my tooth is of no importance to anyone else; and even to myself the resultant ache,—thanks to the mechanical skill of modern dentistry, in the way of finding substitutes for our own teeth,—is almost the only thing of any great significance. Owing to such causes as these there is really nothing to divert us from attempting to measure the mere sensation, if we think of introducing the conception of measurement at all.

When we come to look at the characteristics of the sensation, from this point of view, a little more closely, one of the first things to strike the attention seems to be this. We can get as far as the notion of the *standard* tolerably easily; the difficulty is in proceeding from this to the notion of a *unit.* Something was said about this distinction in the last chapter, and the application is easy to the case before us. A standard is simply an instance, arbitrarily selected, to which we can bring other similar instances for comparison, so as to say whether they are equal to, or greater or less than the standard. I might take my toothache as it was last night at 10 o'clock as such a standard, and by simple comparison I am able to say confidently that the pain is not now as bad as it was then. I might also set up this standard for permanent reference, by resolving that whenever the ache came up to this standard I would give up work and settle down over the fire. In the language of the Act of Parliament which fixes the pound *avoirdupois* we might decree that that particular ache "shall be and be denominated the imperial standard" of such aches for the future.

The defects of such a standard are too obvious to require much pointing out. For one thing it is purely private and personal: I have no means of comparing my standard with

that which others may set up. Of course we all of us make an assumption which really involves such a comparison. Every theory of Ethics upon which we can act involves it to some extent, and the Utilitarian Theory, in its common form of Hedonism, is largely founded upon such a comparison. Unless we took it for granted that others suffer and enjoy in the same sort of way, and to the same sort of degree, as ourselves, we could not know what was our duty towards them, because we could not make the requisite estimate of relative gain and loss. Indeed if there are any duties towards animals the notion of such a comparison must be carried some way in their case as well as in our own.

As Ethics, however, is not generally studied quantitatively, or, if it is, has to decide by averages and large generalizations, considerations of this kind can practically be evaded there. But when as here, the question before us is as to the existence of a standard, the difficulty becomes very serious. In every case where we are able to employ a common standard, we shall find that we have more than one line, so to say, of sensible appeal and verification in regard to it. How do we recognize our pound weight again? Certainly not by its attribute of weight alone. If a number of people were successively sent into a dark room where there were a number of weights to handle, they would not find it easy to ascertain afterwards whether they had handled the same one; and if they did succeed even approximately in coming to an agreement it would only be because a long course of reliance upon other appeals had given them a tolerably accurate notion of the standard. Our means of certainly identifying the standard involve a miscellaneous appeal by various senses to various physical properties. We know the object again by sight and by touch; its size and shape are what we remember: it may be labelled: and as a final security in the case of the State standard, we keep it under lock and key.

In the case of the toothache we have no such resources at hand. Not even the dentist will tell us whether a given tooth is at the moment aching or not; still less how severely it is aching. If it were suggested to take 'writhing' point or 'screaming out' point, by analogy with blood heat or boiling point on old-fashioned thermometers, we should of course be

falling back on an objective standard, and on one not at all more trustworthy than the physical condition of decay of the organ. Turn it which way we will we find it impossible to set up anything approaching to a standard which shall be common to others as well as to ourselves.

Moreover the same causes which make it theoretically impossible to compare correctly the sensations of one person with those of another, make it practically impossible to compare our own sensations at different times. Accordingly a standard selected in the arbitrary way suggested would be of but little avail even to ourselves. If I could recall the ache of last night I might compare it with that of the present moment; but how am I to know that I have recalled the right one? Only by a process of comparison between what is felt and what is imagined, a comparison which is not strictly trustworthy over short intervals of time, and which deteriorates as the time increases. If I could see and handle the ache as I can see and handle the pound weight, and knew that this ache had merely been somewhere else in the meanwhile, just as the pound weight was when I was not lifting or handling it, I should have what I wanted. But when, as here, the sensation to be measured is itself the sole means of identification, an all-important guarantee is wanting.

As regards then the mere conception of psychical standards there does not seem to be any intrinsic difficulty. It is as easy to select them here as it is in the department of physics, and on the same general principles. Only unfortunately there are insuperable difficulties in the way of our making any such use of them in the former case as we can in the latter. As regards the employment of *Units* however the case seems different. It is by no means easy to get a clear conception of what we want in this respect, but when the preliminary difficulties are surmounted, we seem to be in possession of a sort of system of units which, though of a very peculiar nature, do really adapt themselves to a certain kind of application.

(1) The two main difficulties arise in the following way. The essential thing about any unit clearly is that it should be strictly determined. If it be a length, for instance, we must know exactly where to fix the two ends which determine that length. Now mere intensity,—the quality we propose to con-

sider, and that in respect of which there is likely to be most trouble,—resembles a length: it is, so to say, a quantity of one dimension. And in regard to nearly (but not quite) all our sensations there is no difficulty in securing one of the two fixed points,—one of the *ends*, as we may call it, of our unit,—which are needed in order to mark off a definite length. That is, we can in most instances start from a clearly realized *zero*, by our own experience of what it is to have none of that sensation. It is the other end which gives us some trouble; for we nowhere find any kind of natural break or stopping place, or anything analogous to the graduation which we may mark on a scale. Begin with zero, and suppose the intensity gradually increased; we never come to any point at which it seems convenient to mark off a stage for subsequent subdivision; nor do we ever come to a final stop from having actually reached the termination of that growing magnitude.

(2) The other main difficulty arises when we try to *compound* sensations. We saw, in the last chapter, that it was essential to the conception of a unit that a bigger magnitude should be in some intelligible way built up of those which are smaller: on no other supposition can we say that the thing to be measured "contains" the unit such and such a number of times. A plank ten feet in length can be divided into ten pieces of one foot, and each of these pieces can be considered separately and the aggregate be put together and shown to be equal to the original. Similarly, under due explanation, when we are dealing with time and with mass. Any assigned magnitude of these elements admits of analysis and combination. Each portion is similar to the whole, and the aggregate of the parts will,—for the purpose in question, that is, of measurement, for we are not here taking account of any other physical characteristics,—be perceptibly equivalent to the whole. We showed also that the same conditions hold good in other directions, in which at first sight they might seem to be lacking. We are able, for instance, in a perfectly intelligible sense, to compound and resolve velocities, and to say how many times our unit is "contained" in a given velocity.

Can we then be said, in any analogous way, to be able to break up a mere feeling into parts, to consider these separately,

and to re-compound them into a whole? It may perhaps be answered off-hand that if I am suffering from two bad teeth which are apt to ache from time to time, and which seem to me about equally bad when they are at work, that then when they both ache at the same moment the pain must be twice as intense. To such an answer there are two objections. In the first place we are thus ignoring the distinction between extensive and intensive increments of sensation. This distinction may be readily illustrated as follows. Put a finger into hot water, and there are two ways of increasing the pain, viz. by either plunging the finger deeper in, or by making the water hotter. The latter is the true intensive addition which we want to measure, whereas the case of the two aching teeth corresponds rather to the former. And, in the second place, to lay it down that the pain was rendered twice as bad (or whatever adjective we use) would be to beg the whole question at issue. We are then making an inference, and judging what the pain ought to be, instead of deciding by direct consciousness what it is. This would be analogous to deciding how hot we feel by looking at a thermometer. The increment of sensation, and the measure of that increment, must be decided somehow by direct appeal to our own feelings. The unit must be a really subjective one.

III. The device adopted by Fechner, in order to surmount these difficulties, is very ingenious. On his method, what we may be said theoretically[1] to adopt as our unit is the minimum difference of sensation which is just perceptible. In order to

[1] I mean that this is the principle underlying his methods, and, probably, the only one which is universally applicable. As every modern psychologist knows, the formula he adopts is $\gamma = k \log\left(\frac{\beta}{b}\right)$, which slightly departs in appearance from the above description. In the first place we thus abandon the conception of small and discrete differences, and adopt the "limits" of the Differential Calculus. And in the second place, when we compare two sensations in respect of magnitude, we do this, not directly by successive applications of our subjective unit, but indirectly through comparison of the objective stimuli required in order to produce them. In practice this is the only convenient or available method, but it could not be applied to a case like that of the toothache,—which was purposely chosen for this purpose,—where we have no means of measuring the magnitude of the stimulus. If I wanted to find a toothache which should be double a given ache, I could not employ the mathematical formula, because I have no notion as to what β is in this case.

understand what this means, recur to the toothache, and conceive another such ache just a little less bad, but by the smallest difference which is distinctly perceptible as a difference. Or, to vary the phraseology, conceive a number of such aches successively felt (supposing we can retain their intensity in our memory), and that we select the worst of them all which we can distinctly perceive to be less bad than the sample before us. The difference between these two shall constitute our unit for that kind of pain; and the number of such differences by which any assigned pain is built up, that is, the number of such steps required to lead us down to zero, shall represent the magnitude numerically.

There is a good deal to explain and justify here, but the analogy of such a scheme to that of ordinary measurement may be pointed out at once. Suppose that we wanted to measure a long plank, one legitimate mode of proceeding would be as follows. Find another plank which is just a foot shorter; a third which is a foot shorter than the second, and so on. The number of times which we could repeat this process, before coming down to a piece shorter than a whole foot, would of course mark the number of feet in the original plank. The plan would be more circuitous than the common one, of successive application of the same foot-rule, but it would yield the same result.

It would be quite out of our province to go into the details of psychophysical measurement here, but there are two or three difficulties which will so instantly start to the front that they must be briefly noticed at once.

(1) The first of these may take the form of asking in what possible sense we can talk of one sensation as 'just perceptibly differing' from another. Any such admission seems opposed to all physical analogy, where perfect continuity is the rule. To talk, for instance, of one physical length or mass as just differing from another by the least possible amount, would be an obvious concession to loose popular phraseology. However minute the difference, we could always conceivably interpolate another magnitude between the two in question.

In the case of our sensations, however, this is found by experience to be otherwise. When we proceed, so to say, to break off the least possible perceptible portion from a

sensation,—or, to speak more correctly, when we find another similar sensation which differs from the former by the least perceptible amount,—we discover that the apparent physical continuity of a rod or other piece of solid matter does not offer a fair analogy. A more suitable illustration for the purpose would be that of some liquid off which we could not shake a piece smaller than the drop which its physical constitution assigned. Purely psychical measurement may be not unaptly compared to the process of determining the contents of a cup by counting the number of the smallest drops which it would furnish until the whole was exhausted.

All that we can here do is to state the general fact, established by a large and varied amount of experiment and observation, that when any two instances of the same kind of sensation approach, in respect of their intensity, within a certain finite distance of each other, they become indistinguishable by our consciousness.

(2) The general psychophysical law is, that the magnitude of the difference which the stimulus (or objective cause of the sensation) must undergo, in order that the subjective difference between the corresponding sensations shall be just perceptible, varies directly with the magnitude of the stimulus. Or, in terms of a concrete example, if our muscular sensibility is just able to perceive an addition of one ounce on a weight of one pound, then it will just perceive one of two ounces on two pounds, and so on. And these subjective differences, being all alike 'just perceptible', may with some plausibility be considered as being equal, and therefore capable of being employed as units. This general conclusion has been established by very numerous observations over a wide range of experience.

The question therefore naturally arises, Can we, for purposes of measurement, make use of such a unit,—that is, of one where the objective counterparts corresponding to each successive application of what we call the same unit are *not* the same? The answer is that to a certain extent we can, but that several cautions have to be observed in the employment of the plan. Recur to our analogy of the liquid, and make the supposition that the drops vary continually in size as the contents of the glass become larger. If there are two glasses of which one is found to contain 30 drops and the other 60, we can correctly say

that the latter contains 30 drops more than the former. But can we say that it contains twice as much? Certainly not; all we can say is that it contains *twice as many units.* We must carefully keep ourselves to measuring in units only, and we must not imply that the physical magnitudes vary in the same proportion. Of course when the law of correlation between the fulness of the glass and the size of the drop is known we can at once convert one of these elements into terms of the other, and we can thus compare the bulk of liquid corresponding to the second set of 30 drops with that corresponding to the first.

Now this seems to offer a certain analogy to the employment of the unit in Psychophysics[1]. If we find two sensations of the same kind, which are marked respectively as containing 15 and 12 units, it would be perfectly correct to speak of the former as being more intense by three units. But this only means,—when we are reckoning, as here, by "just perceptible differences",—that two similar intermediate intensities could be found such that the three steps, through these, from the greatest to the least, should be just distinguishable. We are not to suppose that a sensation of three units and one of twelve could be in any sense added together so as to produce one of fifteen. This composition is only possible when we are dealing with invariable units, that is, where each successive application of the unit corresponds to a precisely equal increment of the objective material measured. If we are to talk at all of one sensation as being "twice as great" as another, all that we mean is that the process of measurement assigns it twice as many units; we certainly do not mean that the one is brought about by twice as great a physical stimulus as the other[2].

[1] It is meant here that we might if we pleased make the assumption, for certain purposes, that all drops should be counted as equal, and then proceed to reckon in these as units. Of course this would be in defiance of other physical considerations, and would have to be assumed as a mere convention for the sake of convenience. The assumption that all 'just perceptible' subjective differences shall be taken for equal is not in defiance of any physical facts; but then, on the other hand, it does not seem to be an assumption which admits of confirmation by any independent or corroborative evidence. I cannot see my way beyond the recognition of its plausibility and simplicity, and its convenience for assigning numerical values by which sensations can be in a certain way recognized, described, and computed.

[2] A confusion of this kind is found elsewhere for instance, in the case of

(3) It must be noticed that the standard thus selected can hardly claim to be called objective; or rather, if the distinction may be admitted, it is formally but not materially objective. By this is meant that I cannot convey to any one else a correct notion of what my actual standard is, of any particular sensation, but I can put him in possession of a rule by which he can determine the corresponding sensation for himself. If, for instance, two persons were suffering from toothaches which they respectively designate by the numbers 15 and 20, on the method of estimation here under consideration, each would be able to form some notion of how much the other is suffering *on that other's scale*: he knows, say, that his friend is better or worse, as the case may be, than he was yesterday. But there is no intelligible sense in which one of the two can say that he is suffering more or less than the other; at least these measurements give us no assurance of the fact.

So far as this is concerned, any two persons are relatively to each other in the same sort of position as would be occupied by (say) an Englishman and a Frenchman who could each speak the language of the other, but who did not know what was the area respectively of an acre and a *hectare*. Each could explain whether his crop was greater or less than it was last year, and each can translate his own measures into terms of his own experience, but he has no means of comparing the fertility of the land cultivated by the other with that of the land which he cultivates himself. This state of things is of course inevitable in every case where we attempt intercommunication about our simple feelings, but it makes itself naturally most felt when the question is one of *measuring* such feelings.

(4) The remaining characteristic of such a scheme of measurement which deserves notice becomes of less importance than it would otherwise possess, after the admissions we have just made. It is obvious that we cannot avoid a multiplicity of

temperature. Some people cannot rid themselves of the notion that it ought to be 'twice as hot' when the thermometer stands at 90° as when it stands at 45°. We have to point out to them that what has been doubled is not our sensation of heat, nor even the heat itself, but only the number of degrees on the scale, or the corresponding length of the mercury column. We *can*, no doubt, speak of 'doubling' a given quantity of heat, but this leads to a very different enquiry from that of temperature as shown by a thermometer.

units. In fact every distinct sensation involves a distinct unit, namely the least perceptible difference in that kind of sensation. We saw, in the last chapter, that primitive measures of weight, volume, length, &c. had started from a bewildering variety and multiplicity; but that Law and Science were at work, doing what they could to introduce one universal system for each class of measurable elements or abstractions. And such a step is very important, because we are perpetually having to compare things of different kinds in respect of their magnitude; and, what is more, not only to compare them but physically to compound, divide, and adjust them to each other. A lens made in Paris on a scale of metres may have to be fitted into a tube made in Birmingham on one of feet; fruit may be bought by the kilogramme in one country and sold by the pound in another. In other words, where physical division and composition and adjustment are required, and where accordingly we work with a unit of fixed magnitude, it becomes exceedingly important that the unit should if possible be a universal one. But where we cannot cut a thing up into pieces, and join these pieces together, or actually lay one thing alongside another, 'measurement' becomes a very subordinate matter, and such a unit as we have described above will answer our purpose very fairly.

CHAPTER XX.

ON CERTAIN FOUNDATIONS OF MATHEMATICS.

THE DATA OF GEOMETRY.

SUCH discussion of the foundations of geometrical reasoning as has hitherto found a place in Treatises on Induction or on Philosophy generally, has mostly been directed into one channel. What has been discussed has been mainly the origin and nature of the certainty we feel about the axioms and conclusions:—have these been derived from within or from without? do they differ in kind, in respect of the certainty we feel, from the axioms and conclusions of other branches of knowledge? and so forth[1]. Of late years the dispute has shifted to somewhat new ground. In the pre-evolutionary stage, when the ultimate test was considered to be that of simple introspection, and the question actually in dispute was little more than this,—Can the certainty we now feel have grown up by association in the life of the individual? it was very different. The dispute was by comparison a narrow one, and the issue not very doubtful. At least we cannot regard the arguments of J. S. Mill (the most distinguished of the recent supporters of non-evolutionary empiricism) in his System of Logic, as really satisfactory. Now, however, it is very different. The matter cannot reasonably be discussed, as a sort of side issue, in a single chapter of a work on Inductive reasoning. Nor is it necessary that we should discuss it at all. Formerly, the mere

[1] Recognition of this topic seems inevitably called for here, if only because most of the works which the reader is likely to have studied contain some such discussion at this stage. But at the risk of destroying any trust the philosophical reader may entertain about my right to touch these questions at all, I must frankly admit that I cannot claim to have arrived at any confident judgment upon the matter. Fortunately, as above remarked, such a confident or final judgment is not necessary for present logical purposes.

admission that the certainty we feel, is, so far as introspection can furnish a test, absolute certainty; that is, that by no effort can we conceive or imagine the facts being otherwise, was an admission which at once committed us to one side or the other in the debate, or was at least a potent factor in so doing. And since the question was forced upon our notice, it was but reasonable that we should be called upon to give an answer one way or the other. But now we are presumably all agreed about the subjective or individual certainty. No one supposes that any possible selection of experience to which an infant could be subjected would avail to instil geometrical axioms different from those which we all recognize.

What it is here proposed to discuss is something rather different from this[1]. Our enquiry does not so much refer to the origin of geometrical axioms as to the nature of the geometrical subject-matter, namely the lines and surfaces with which we there deal. To what extent, and in what way, do these differ from the elements of experience from which we start in our ordinary inductive reasonings? And how are we to account for the peculiar discussions and difficulties to which they have given rise?

I. The first point then which we propose to discuss here is the nature of the subject-matter of geometry: that is, to enquire what exactly are the surfaces, lines, and points with which the geometer has to deal in his investigations. The best short answer that can be given is, that, as regards their general nature, they are abstractions from things which actually present themselves to us as existing; and that, as regards their quality, they represent improvements or simplifications upon things which exist; such improvements and simplifications being

[1] The discussions in this chapter may be passed over without serious loss by the non-mathematical reader. But the various difficulties involved are so widely felt by those who read mathematics intelligently, that some attempt to solve them logically ought to be made. I am only too well aware that many advanced modern mathematicians,—especially those who have reached the enviable position of accepting the absolutely infinite,—will reject such attempts at smoothing our difficulties as are here made. Still what has brought help to one mind may be offered without impertinence (if expressed without dogmatism) to other minds. One young Cambridge student at least, many years ago, found the various problems here discussed bristling with difficulties which seemed to be contemptuously ignored by his teachers and his text books.

conceived as carried out to perfection. These remarks will need some little explanation and illustration.

In saying above that the surfaces, lines and points of geometry are *abstractions*, the doctrine which we most prominently have in view as one to be rejected, is that which is presumably the prevalent unscientific opinion, in accordance with which these elements are regarded as being a sort of entities which can exist apart. Such an opinion is probably greatly encouraged by the large resort necessarily made to the process of tracing our geometrical figures upon paper when working out problems. Even Mill seems to suppose that this is the only alternative open to the geometers, when he maintains that it is impossible for us to conceive such a thing as a perfect line or point. Any such impossibility rests upon the common habit of regarding the lines and points as being, so to say, substantive entities which we must try to conceive as existing by themselves, instead of being attributes, that is, boundaries, of something else, namely of solids. Presumably a very thin sheet of paper, a very fine thread, a very small particle, are taken respectively as first approximations, and then the effort is made to fine these down as far as possible so as to obtain what we want. To 'draw a straight line' from one position to another is to conceive an exceedingly thin spider's thread stretched between them. Or perhaps, in accordance with an ingenious suggestion which has been offered, in order to attain to a due conception of the geometrical line, we are recommended to picture to ourselves the finest conceivable thread, and then to imagine the central line down the middle of this thread. It need hardly be remarked that any attempt in this direction must be a failure. Do what we will in the way of refinement, any plane arrived at in such a way continues to have both an upper and an under side, namely to be a lamina: the line continues to have a surface all round it, namely to be a cylinder of some sort; and the point continues to have a surface all over it, that is, to be a closed solid of some figure or other.

The soundest way of securing what we want is to begin with the solid. This must have an outside or surface; it is with this outside or surface that the geometer deals, and it is this which, when perfectly flat, forms his 'plane'. We must regard it, not as an indefinitely thin lamina or sheet, but simply as the

outside or boundary of a solid. It is therefore not so much a material entity as a sort of attribute or quality of a solid, reached by an act of abstraction. A surface thus conceived seems fully to come up to the required condition of having extension without any thickness. To say, as is sometimes said, that what we thus do is to *attend* merely to the surface, seems hardly to say enough. We can surely imagine or conceive it clearly enough for our purpose.

Having thus obtained our true geometrical surface, our next step, namely that by which we obtain the *line*, is obvious enough. The line is merely the boundary or outline of the surface, just as this is the boundary of the solid. It is not a thing which can be drawn on the surface, but it is one of the boundaries of the lines which we attempt to draw on the surface, but which are in reality nothing more than portions of that surface, that is, narrow strips or paths upon it. The surface of, say, a sheet of paper being a plane, if we cut off a portion of it this gives us an outline to the surface, which is itself a line. Like the surface, it fulfils its geometrical qualification, for it has length, but no breadth, and the superposition or juxtaposition of two such lines (to be presently noticed) does not produce any breadth. Finally, on the same explanation, the point is the boundary of the line; or, to use a more familiar word in this case, it is the end of the line. The line having only length to begin with, the end of it will of course have neither length, breadth nor thickness. We may therefore sum up by saying that the surface is the outside (or inside) of a limited solid: the line is the boundary of a limited surface; and the point is the end of a limited line.

The best familiar illustration of this view is given by reminding the reader that every ordinary diagram in Euclid really contains at least *two* complete diagrams, of slightly different sizes, according as we regard the true geometrical lines as being assigned by one boundary or the other of the narrow channels or paths constituted by the so-called lines which the wood-cut on the page sets before the eye.

It may perhaps be objected that to proceed as above is to invert the proper order. No doubt the procedure adopted in many mathematical discussions is exactly the opposite, for they start with the point as supposed to be given and conceive the

line as being produced by the continuous motion of such a point; and similarly, at the next stage, they start with the line, and conceive that this by its lateral motion traces out a surface; and finally the surface may be conceived, by its motion, to yield a solid. It is quite true that this is much the more familiar process in geometry, but there is no real incompatibility between the two different modes of regarding the same set of elements. To find such an incompatibility would be to confound the psychological processes by which we are supposed to have originally found, and subsequently become able to justify, our conceptions, with the logical processes by which we may compare and work out further results from them. In our geometrical speculations there is nothing to hinder us from starting with such abstractions or derivative elements as points or lines, without stopping to explain how they had been derived, or how they have to be presented to the mind when we endeavour to realize them in their strictest form. And, thus starting, we may, for the purposes of exposition, derive from them the very results from which they had been themselves originally acquired. Every mathematician knows what a convenience it often is to take the point for granted as originally given, and to enquire what sort of line it will trace out if it moves under assigned conditions. We have, also, already (when describing what are called Genetic Definitions) indicated how simple and convenient a definition may be given of certain solid figures by supposing that they have been produced by the assigned motion of a surface or line of known form.

So far we have considered only one of the common objections against the possible existence of these geometrical elements, namely that their definitions, strictly regarded, involve contradictions. But there is a second objection which must now be noticed, namely that the geometer attempts to bestow upon these elements perfections which no experience can yield nor therefore any effort of the mind conceive. Let us grant, it is urged, that there could exist surfaces with extension and no thickness, in the way above suggested, yet the geometer wants a perfectly smooth surface for his plane, whereas every experienced, and therefore every conceivable surface, is more or less rough. Similarly there can be no line which is really straight, no circle which is accurately uniform in its curvature,

nor in fact any figure which does not fall short of its professions. Mill, amongst others, has urged this objection, and has in consequence laid it down that geometry is founded upon hypotheses or assumptions which are not accurately true.

In replying to this objection we must begin by clearing the ground. In the first place, is it so certain that there are no perfect surfaces or lines in existence? Those who assert this say a good deal more than we really know about the nature of molecules. So far from its being known that these fall short of that perfection of surface and of shape which the geometer requires, it is often taken for granted that they possess these perfections to the utmost degree. What must be meant therefore is that the surfaces and lines exhibited by bodies of sensible magnitude fall short in these repects. This, of course, must be fully admitted. The finest 'Whitworth plane' which the trained eye and touch of a skilled mechanic can produce, is doubtless covered with irregularities. Even supposing that we could not detect these by the microscope, we might reasonably infer them from the fact of friction between two such planes being unavoidable.

Such an admission as this does not seem however to carry us very far on any rational interpretation of Empiricism. No doubt if we accepted the crude view of Hume that every impression was derived from a sensation, in the sense that every concrete object which the mind can imagine must be framed of elements which had been actually observed either singly or in some other combination, the case of the geometer would be a bad one. But this is surely no necessary consequence of the Empirical account of our knowledge. The way I should look at the matter is this. Take one of these defective geometrical elements,—the 'rough plane' for instance,—and consider the nature of that roughness. How do we regard this plane? Presumably as a surface covered with small irregularities. But what is the nature of any one of these irregularities? It is itself a surface, and therefore the same question may be repeated in reference to it. But there seems no occasion to suppose, from anything we know either of the constitution of bodies or of our powers of conception, that this process should have to be repeated *in infinitum*. It seems quite as reasonable to suppose, and decidedly easier to conceive, that if we could

thus continue to diminish the area contemplated and also continue to magnify it, we should come down at last to minute fragments of perfectly smooth surface, whatever their actual shape might be. In a word, ultimate elementary smoothness seems to be the simplest and easiest assumption we can make.

We are talking at present, of course, of what can be conceived. Standing on this ground it seems to me that the case is much the same whether we suppose matter to be continuous or discontinuous. If we take the latter alternative,—in other words, adopt the common view of physicists that matter is an aggregate of molecules[1],—we have to enquire what sort of shape we conceive those molecules to possess. The easiest and simplest course, whether physically correct or not, is to conceive them as spherical, or at any rate as of some perfectly true geometrical figure. If, on the other hand, matter were continuous, the result, so far as our powers of conception extend, would not be very different. Why do we suppose any given surface to be rough or irregular? Because experience tells us that it is so. But if we suppose ourselves continually looking closer and closer into smaller and smaller portions, this testimony of experience fails to hold good. So far as our powers of conception go it seems the simplest and easiest plan to picture to ourselves this irregular surface as built up of innumerable minute facets of perfectly smooth surface.

If this be so, all that the geometer requires to render his data perfectly accurate seems to be granted. We can conceive, say, a portion, however minute, of surface perfectly smooth, in the sense of possessing no irregularities. If this be supposed perfectly flat also, like the face of a crystal, all we have to do is to suppose it enlarged till it is as big as we want. To deny the power of doing this would be to tie down the imagination, not merely to the limits of common experience in a wide sense, but to the precise limits which our own senses had hitherto assigned in respect of concrete objects, and this without the right of analysis or of extending further such qualities as we had only actually witnessed up to a certain degree.

The fact is that much of the ordinary reasoning on this

[1] Recent speculations, in accordance with which the atom is itself conceived to be composed of a multitude of much more minute particles called *ions*, does not affect any value there may be in the above suggestions.

subject seems to have a tendency to play fast and loose with experience. The first impression of the plain man, relying on the immediate data of sense, is that some surfaces,—say that of a piece of plate glass,—*are* perfectly smooth planes. But the superior person interposes here, and convinces him, partly by better experience (such as that of the microscope) and partly by general reasoning, that he was mistaken, and that every such surface is rough and irregular. But these appeals can only carry us a certain way, and when we push our enquiries further on we come to a stage at last at which perfect smoothness or regularity seems quite as consonant to experience, and much more easy to conceive than roughness. If any one thinks this is not so, he may be asked to try to conceive a surface such that successive magnification carried on for ever only displayed new and endless orders of 'roughness'. He will surely find this the more difficult and gratuitous supposition.

So far it does not seem that any very serious difficulty is involved in the attempt to conceive and treat our geometrical elements. The only consequences which have caused me much perplexity are of a somewhat different kind, and as they may have occurred to others also they may be briefly noticed here.

It may seem then that a direct contradiction is involved in the attempt thus to conceive a line, inasmuch as we may be required to endow one and the same line with contradictory attributes, in case it happens to be the boundary between two surfaces possessing contradictory attributes. Take, for instance, a circle, and suppose it to be divided in two by a diameter, and let one half the surface be black and the other half white. Is the common dividing diameter to be considered black or white? If a line were regarded as a thread, however fine, of course it must be of one or the other colour; and to make it either alone would display partiality. Nor does it seem to me that any way of regarding it as a *limit*, that is, as the ultimate result of the indefinite thinning down of a thin slice of the circle, would avail to escape the contradiction. But if we regard it as a mere abstraction, that is, as a pure boundary of a surface, there is no risk of endowing it with contradictory attributes, because those attributes do not really belong to it. Colour does not properly belong, like shape, to a mere boundary. We are scarcely conscious of this fact so long as we think only of a single

surface. We may say, for instance, that every part of a sheet of paper being white, the outside or boundary line must be so too, and we do not at once see any difficulty. But when we remember that the same boundary line belongs to whatever may be in juxtaposition with that sheet of paper, we are reminded that we had no right to predicate colour of it at all.

We may picture the result as follows. We know that if one snaps a hard biscuit or thin piece of polished stone, and puts the two pieces in contact, the two boundary lines coincide in one and the junction becomes invisible. This ought to be so, since neither has any width. Now conceive one half of the broken surface to be painted black, the other being left white. We are apt to say that the two boundaries must be respectively black and white, because the surfaces which they bound are so. But join the two surfaces, and the so-called black and white lines coalesce in one.

Identically the same boundary line, then, belongs to both the surfaces. The case is in fact analogous to what we ought to consider as existing in respect of *time*. A true 'instant', like a line, divides two portions of time, and belongs to one of these exactly as much as to the other. Midnight, for instance, —astronomical and legal conventions apart,—is just as much 'a part of' the Sunday before (say) as of the Monday after. What then ought to be meant by the statement,—occasionally made for our perplexity on suburban pillar-boxes,—that there being a clearance every week-day at midnight, there is no clearance on a Sunday? To omit one clearance only, in pursuance of such a notice, (as is actually done) is an obvious impropriety: to omit two would be a little more consistent, but would lead to the absurdity that the intervening Sunday claimed for itself alone both its boundary lines, leaving none for the Saturday and Monday. The only rational answer is that when we subtract a whole day of 24 hours, so to say, with its two boundaries attached to it, we none the less leave those same boundaries behind, because they belonged just as much to the adjacent days. In other words, if a whole day be dropped out, for letter distribution purposes, there are still seven midnights left behind. Any other answer supposes that an 'instant' of time is a *portion* of time: the last second, or fraction of a second, just before or after the line of division.

It may perhaps be suggested that, in the example of the circle, given above, we are merely shuffling out of the difficulty, by declining to make any predication as to the colour of such a thing as a boundary line. The following problem, it may be urged, being one in which physical consequences are involved, will force us to an answer one way or the other, without giving any opening to evasion.

Imagine, then, that the circle, instead of being coloured half black and half white, is half rough and half smooth; these qualities being interpreted in their ordinary mechanical sense. Set it on a horizontal line, and suppose that it is induced to move along this line, in its own plane; will it roll or slide? Clearly, if placed with any portion of its rough circumference in contact with the line, it will begin to roll: if with any portion of the smooth, it will slide. Suppose now that it is placed with the diameter which divides the two halves at right angles to the line, so that the point of junction of the two semi-circumferences is the point of contact: which will it do? It must adopt one course or the other; so that no evasion is possible, here, equivalent to our denial of the applicability of the property of colour to a mathematical line.

Before answering this question, let us glance at the following, which like all logical puzzles, however trifling is worth clearing from its perplexities. Is the greatest weight a man can raise the same as the least which he cannot raise; and if not what is the difference between them? The question is so far the same as before that we are here also contemplating the existence of a sort of boundary; only in this case it is not the limit of a surface or line, but the limit of a mass as defined by its property of gravity. The difficulty and contradiction arise when we contemplate this boundary as something existing apart from that which it bounds. The physical answer here would be that there is a certain weight which the man could exactly balance (physiological possibilities of constant strength being granted): any superior weight, down to this inclusive, he cannot lift; any weight whatever, below this, he can lift, however near to the limit. In this case the boundary is necessarily assigned to that which cannot be lifted. To talk therefore of a greatest weight which can be lifted is to postulate something to which no accurate signification can be assigned. The

assumption involves the same absurdity as to talk of the smallest possible angle: choose an angle as small as we will, another can be chosen or conceived which shall be smaller still.

By analogy with this example we may suggest the following answer to the problem about the dividing circle. Place the circle with any point of the smooth portion, *up to the dividing line inclusive*, in contact with the line, and it will slide. Place it with any point of the rough portion in contact, *as near to the dividing line as we please*, and it will roll. The angle through which it will roll, before the smooth part comes into contact with the line on which it rolls, will of course become less and less as the point from which it was started approaches the dividing line, and this without limit. When the point of starting reaches that dividing line the angle of rotation will be zero: that is, the circle will not roll at all[1]. This answer involves no actual contradiction, and does not seem to raise any difficulties beyond those which have to be surmounted whenever we deal with a geometrical 'limit'.

II. The next important question for discussion may be introduced by the enquiry whether the objects which form the subject-matter of Geometry,—its surfaces and lines, that is,—are to be regarded as *given* to us, or as *constructed by* us. The remarks made during the last few pages might possibly be understood to imply that these objects belong to the class of phenomena which nature presents to us, and that all which we contribute is a certain finish and perfection with which we endow them. I think that this is so with certain surfaces and figures; for instance the sphere, which is constantly presented to us in the form of a drop of any liquid, and the circle, in that of the disk of the full moon. And unless we had had such a basis of physical figures actually presented to us from time to time, from which to start, it is difficult to see how any progress could ever have been made in the way of Geometry.

But in the case of far the majority of our surfaces and figures we can only regard them as being our own constructions. By this is meant here of course what is generally meant in Geometry, that is, that we draw on paper or elsewhere a figure which more or less closely resembles what we want, and use

[1] This problem was, as it now seems to me, not quite correctly answered in the former edition.

this as an aid to the more perfect mental construction in which all errors and irregularities are supposed to be removed.

The importance of this distinction is much greater than might be supposed at first sight. This importance does not consist however in the mere difference of origin, but in a consequence of this difference which affects a certain class of these figures, those namely which are commonly described as infinite, in contrast with those which are finite or closed. I cannot but think that a very large proportion of the puzzles with which even some mathematicians are troubled, and at which most non-mathematicians are either scandalized or lost in admiration as the case may be, arise out of a disregard of this distinction.

Take for consideration any curve which has infinite branches, for instance the hyperbola. There is, I apprehend, a notion which if vaguely held is still widely spread, that all which the geometer does is to trace out the curve so far as he has occasion to go, but that the curve itself 'exists' in exactly the same sense beyond any point which he may reach, as it did before his reaching that point. The curve itself is regarded as infinite, though all that we produce or contemplate of it is finite. If so, the term infinite must clearly be understood in its absolute sense, and not in its only acceptable relative sense, in which it merely indicates the capacity of going on as long as we please. I should say on the contrary that the curve does not exist in any sense whatever beyond the point to which we may have chosen to trace it. If any one replies that it exists potentially, the answer is that so far as this sort of existence is concerned every curve exists, of every relative size and in every position and direction, on every plane; for we may always conceive ourselves commencing to draw lines there as we conceive ourselves continuing to draw the branches of the hyperbola. I cannot but regard the customary way of speaking of such curves as liable to mislead. We talk for instance of no finite space being able to contain the curve, when what we mean is that we can extend the curve up to the limits afforded by any given finite space, if we choose to do so.

Directly we thus regard any of the infinitely extensible curves as being absolutely infinite, a multitude of contradictions, of the kind of which Hamilton (*Metaphysics* II. 527) has gathered samples wherewith to perplex himself and some of his readers,

soon present themselves. Each branch of the hyperbola ultimately becomes a straight line, but never really is one. It ultimately coincides with its asymptote, but is always distinguishable from it; and so forth. Understand however that 'infinity' simply means the right to go on as long as we please, that coincidence with an asymptote means getting as near to it as we please, and so on, and all the insurmountable logical difficulties seem to be removed.

It deserves notice that no similar difficulty has been experienced about the straight line. Thus we readily admit the expression 'a finite straight line,' whereas 'a finite hyperbola' would be regarded as a contradiction in terms. There is not, however, the slightest geometrical difference between the two cases. Every straight line is infinite in the same sense as the hyperbola is infinite, namely in the sense that, beyond the portion we have before us, we can add on any further portion we please. But partly owing to our familiarity with material, and therefore limited, straight lines, partly also to the fact that the extreme simplicity of the straight line does not raise any perplexities as to its behaviour 'at infinity', (for it is the same there as where it is under our notice) the difficulties hardly exist here. We recognize at once that any 'straight line' is naturally finite, and that its infinity merely consists in our power to 'produce' it, as Euclid says, as far as we feel inclined to do so. Neither more nor less than this holds true of the hyperbola, and of every other curve with infinite branches.

This necessity of regarding geometrical figures,—at any rate those of the non-closed description,—as constructions of our own which we can carry out to any point we like, may be put in a far stronger light. So long as we deal with the more familiar examples of curves with infinite branches, such as the hyperbola, it is difficult to bring the question to a definite issue. The explanation given above seems to me the only rational one, but absolute infinity in a line is so inconceivable to us that those who admit its possibility can hardly be confuted by such examples. Take then the instance of the Archimedean spiral, which may be familiarly illustrated as being of the form of a watch-spring which at every point bends at the same angle towards the centre; and see what follows from supposing that

this curve consists of anything more than what we choose to construct, or conceive ourselves constructing, of it.

Start from any point in it and proceed for ever along the curve, *away* from the centre, and we have no further difficulties than those which must always attend the assumption of infinity as actually given to us. They are of the same nature as those discussed above in the case of the hyperbola, and need not be repeated here, since they are avoided so soon as we admit that the curve 'exists' no further than we choose to construct it. But start from the same point and proceed *inwards*, and we are landed in a rather startling contradiction, for it is a contradiction not involving an infinite length but a strictly finite and therefore intelligible length. The difficulty comes about in this way. The entire length of the spiral, thus measured from any point inwards, is finite[1]. But a finite line surely must have an end. Where then is this end to be placed? Its only possible position would be the centre; but by the very process of construction, by which we are supposed to make an endless number of revolutions in tracing the curve, that centre can never be reached: it cannot even be reached by the suggestion of any possible increase of rate in making these revolutions as we proceed to trace the curve.

The process of construction,—namely that of making an endless number of revolutions round the centre,—is one which we can never regard as having been completed. And, as we are dealing with a finite length, it is easier to see here that the curve *is* a construction of ours and nothing else.

A somewhat different mode of exhibiting the contradiction may serve to make it plainer. Conceive a moving point to start from some given position on the spiral, say the original position from which we started to measure the length, and suppose it to move with constant speed. The distance travelled (s) is a function of the time elapsed (t). The equation connecting them is, of course, $s = vt$. As before, no demonstrable contradiction is elicited by supposing that the curve along which the point moves in the *outward* direction is infinite. Take

[1] That is, the rectangular spiral $r = ae^{\theta}$, which makes an angle of $\frac{1}{4}\pi$ with its radius at every point. If we measure the length of the curve (s) from the point determined by $\theta = 0$, we have $s = a\sqrt{2}\,(e^{\theta} - 1)$; which is infinite measured outwards, and finite ($= a\sqrt{2}$) measured inwards.

any period, however long, and the point will have traced the distance $s = vt$ in the course of that time. But starting inwards we find that at the end of a period $t = \frac{a}{v}\sqrt{2}$ there would be no more space left for the point to traverse. Apparently then the consequence follows that it *must* by then have reached that termination of its path for which we find ourselves unable to assign any exact position, and whose existence therefore we are prompted to deny.

There seems however no difficulty in all this if we understand that the spiral is nothing more than a line traced according to certain conditions, and that the amount we have at any time traced is the amount there *is* of it. We may conceive our obtaining the curve by the following construction. Take a straight line limited towards one direction, but which may be extended as we please in the other; and taking any fixed point outside of it as our centre or starting point, bend the line in such a way that it always slopes at a fixed angle towards that centre. As we proceed outwards we should never come to an end: we should have to call for more and more of the line without cessation, as we went on, thus constructing our spiral. As we proceed inwards, we should also never come to an end, but from a different reason, namely, because we could never, by our proposed process of constructing the curve, use up all the length of line allotted us[1]. The pieces we should successively call for in order to make each fresh revolution, or any part of each revolution, would be continually less and less, and would never exhaust the whole length assigned us. So regarded there seems no real difficulty involved in the construction.

The spiral curve thus produced by a continuous process of taking smaller and smaller *curved* pieces should be compared with the result produced by continually prolonging a given straight line by taking smaller and smaller *straight* portions. Most persons are familiar enough with the statement that if we take a point, say, one inch from another point along a straight line, halve the distance, halve it again, and so on everlastingly, we should never reach that second point. But this is clearly only another way of attempting to construct a straight line from one point to another by continual addition

[1] We suppose of course that the point of starting had been properly chosen.

of smaller and smaller pieces. And by such a process the whole straight line never could be completely drawn. Of course we should never adopt such a mode of drawing it, because we have the alternative method of drawing straight lines directly in one piece. But if we could not adopt this ready mode of doing it, and were confined to the method of endless approximation, we should then be in just the same position as regards this straight line that we actually are in as regards the spiral.

III. In connexion with this a difficulty must be noticed which seems at one time to have been felt as a very real one, and which has seriously misled even such thinkers as Berkeley and Hume. According to them (I quote the words of Hume, *Treatise of Human Nature*, Part II.) "Whatever is capable of being divided *in infinitum* must consist of an infinite number of parts, and that it is impossible to set any bounds to the number of parts without setting bounds at the same time to the division." Berkeley had previously used almost the same words, with a different conclusion: "It follows that there is an infinite number of parts in each particle of matter" (*Principles of Human Knowledge*). From this assumption, combined with that of the essential finitude of the human mind, Hume draws the conclusion that in the attempt to subdivide any line in imagination we should necessarily come down in time to a certain minimum of which we "cannot conceive any subdivision, and which cannot be diminished without a total annihilation". We are not concerned here so much with the soundness of his reasoning, which seems to me extremely intricate and at times contradictory, as with the above quoted assumption from which it starts. It would be impossible to express more clearly a view which is very often involved more or less in the language employed in speaking about geometrical figures, and which in so far as it is deliberately held seems fatal to any clear views about the nature of curves or surfaces.

The expression that a line "consists of parts", however convenient when duly explained, seems distinctly inaccurate and extremely liable to confuse. Doubtless every line consists of them, as soon as we have divided it, but not in any intelligible sense before we have done so. The division is an act of ours, just as was the original construction of the line, and the error

is of the same kind in each case, that is, of supposing that something already exists because we have the power of introducing it. My penholder consists of two halves, and of four quarters, in the sense that I may mark or divide it at any such points; but in the same sense it consists of every other fractional part which the imagination can conceive. It consists similarly of a certain number of $\sqrt{2}$-inch parts, for I might measure off as many of these as would go into it; and so on without any limits to the suggestion of possible divisions or constituent 'parts'. But there is not really any 'part' except when a thing either is, or is conceived to be, parted. The confusion created by language of this kind is, I take it, at the bottom of very many of the difficulties commonly felt about the principles of the Differential Calculus[1].

IV. This brings us to the last point which we can afford to consider in a general treatise such as this; namely, the sense in which we are to understand the so-called "infinitely small" in Geometry and Algebra. The confusion which has been bred from this source is far more serious and persistent than from any of those discussed above, and it is probable that some distrust still lurks in the minds of many students about the principles and methods of the Differential Calculus. There are doubtless not a few, even amongst those who can go through the calculations successfully, who nevertheless cannot quite suppress a doubt as to whether their conclusions are as absolutely and rigorously true as any of those admitted by Euclid and the ordinary Geometry. Readers of English philosophy know how staggered Berkeley was with the Method of Fluxions, as he understood it; and considering the terms so often employed in expounding that method one can scarcely wonder at even a man of his order being somewhat puzzled at first, though it has always been a subject of regret with me that he should have rejected the new processes so strenuously to the last.

The main distinction which we have to keep in mind is that

[1] The conception here seems fundamentally the same as that which is involved in the old discussion (Aristotelian or Platonic in origin) about the pre-existence of the statue, say of Hermes, in the block, before the sculptor cut it out:—that all he had to do in fact was to remove what was *not* Hermes. Hallam (*Literature of Europe*, III. 374) referring to geometrical figures says "they exist in space, to repeat the metaphor (which indeed is no metaphor but an instance), as the statue exists in the block".

between a certain magnitude, whatever it may be, which we seek to determine, and some other magnitude which we may find it convenient to employ in the process of determining it. The former is, generally speaking, finite and perfectly definite: whereas the latter may be one which can never be regarded as actually completed, though the result aimed at by it can be clearly and accurately calculated.

Put the following case. Suppose the width of a certain door-frame had to be accurately determined by a given measuring rod, I should of course assume that this rod was of a precisely fixed and determinable length. If, however, I found that it was slowly but continually contracting in length, I might at first conclude that accuracy of determination was impossible with such an instrument as this. The utmost we could do, it would be thought, would be to wait till the contraction was supposed to be nearly over, and then to assume that the consequent error was so small as to be negligible. We should in fact treat it like the thermometer tube mentioned on page 443. No other resource indeed would be available in common geometry, where we are supposed to measure by mere superposition. But now suppose that I were able to show that the door-frame happened to be precisely the 'limit' towards which the rod was continually contracting; and suppose also that I had some means of determining with equal precision what the actual length was towards which that ruler was tending, then the frame could be measured with exactly the same accuracy as by a fixed rod.

Of course such a method as this is circuitous, and its practical utility is only found in its applicability to certain cases in which no direct method is available. But it will be convenient to commence with an example in which both methods, the direct and the indirect, are equally available, in order to show that one may be precisely as accurate as the other. Take then this case. The hands of a clock are coincident at twelve o'clock: when will they next coincide? The desired position,—in other words, the time-magnitude at which we are aiming,—is a perfectly definite and determinable one, and may be obtained at once by simple arithmetic, or by an equation which only involves simple arithmetic. The long hand marks off the minutes twelve times as fast as the short hand. Therefore, if x represent the number of minute-intervals

after twelve where the coincidence occurs, we must have the distance travelled over by the one hand twelve times as great as that travelled over by the other. That is, $60 + x = 12x$, whence $x = 5\frac{5}{11}$; that is, the hands will again coincide at $5\frac{5}{11}$ minutes past one o'clock.

The result so obtained is of course perfectly determinate, and is given by a method which does not suggest the slightest defect from accuracy. But it so happens that there is an alternative method of obtaining the same result. We might proceed as follows. When the minute-hand has reached one o'clock, the hour-hand has reached $1\frac{1}{12}$ of an hour; the former has therefore somewhat further to go. When it has reached the point thus indicated, the hour-hand has got on still further, viz. the $\frac{1}{144}$th of an hour. And so on, as long as we conceive ourselves to carry on the process.

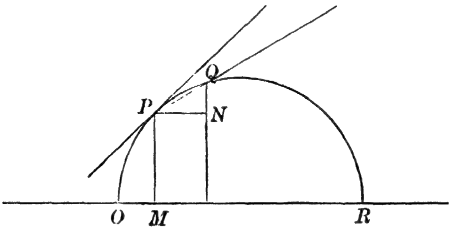
The object of recalling this ancient logical puzzle in a slightly different form,—(for it is Achilles and his tortoise over again)—is to remind the reader that we are here employing it for a purpose somewhat different from that which it commonly subserves. What the example here shows is that a result of a perfectly precise and definite kind may be equally well obtained in two ways: either by a straightforward and obviously finite process, or by one which appears as though it could be indicated but could never be completed.

Look at this a little closer. There is an erroneous way and a sound way of stating the latter process. As regards the erroneous way, we may describe ourselves as trying to complete the process directly; but, finding this impossible and stopping short at some point, we then declare that the remainder was so small that it might be neglected without fear of sensible error. The accurate way is to insist that we do not attempt to 'sum' an infinite series; for this cannot be done. What we really have in view is the quantity towards which that series indefinitely tends, and to the attainment of which the series is a convenient means. In a word, the result we want to attain is $5\frac{5}{11}$ minutes: that precise result and no other. We could obtain it by a simple and direct process, but we here prefer another mode. This mode, loosely stated and conceived, seems as if it involved the impossible task of summing up an infinite number of smaller and smaller elements, and then of evading the impossibility by actual inaccuracy, namely by

stopping when we think we have done as much as is safe. Rightly conceived, we are throughout arguing about the same determinate result as was shown to be directly obtainable, and we only use the infinite series as a scaffolding for obtaining it. We say in fact, Such a series as that before us can be shown to tend indefinitely towards the quantity we want; moreover the value towards which it tends is precisely calculable; accordingly the quantity we want is thus precisely calculable.

We may conclude this elementary introduction to the main principle of the Calculus, by one or two examples of a more usual kind than those given above, and dealing directly with geometry. We purposely select examples so simple that either the direct or the indirect process is available.

Suppose it were desired to calculate the position of a tangent line to a given curve, at a given point; say that of the circle $OPQR$ at the point P.



Here, as in the case of the clock-face, there are two modes of setting to work. We may proceed directly, by what may be called a finite process, such as that which Euclid adopts. If OP were, say, one quarter of the semi-circumference, we should find by well-known methods, that the position of the tangent at P was assigned by its being inclined at 45° to the base line OR.

But there is a different way of setting to work, as follows. Take another point Q in the curve, and draw the chord PQ; and suppose that the point Q continues to move nearer and nearer, without any limit, to P. It is very often said that *when Q has reached P* we shall have the tangent required. But this is an inaccurate way of speaking. By such a process, strictly interpreted, we can never suppose Q to reach P, for if it did there would only be one point, and not the two points

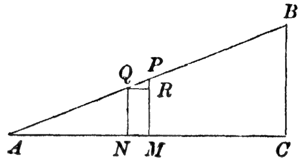
necessarily required for drawing a chord. What is meant is that such a process, namely that of the gradual indefinitely close approach of Q towards P, is merely a sort of scaffold-process, not for giving but for *indicating*, the definite tangent line we want. That tangent line corresponds to the desired hour on the clock-face in our former problem; and the gradual approach of Q to P corresponds to the indefinitely numerous steps by which we supposed ourselves to tend towards that hour. But in each case alike the result can never be actually attained by such a process. All that we do is to show that the result towards which we continually tend, and the result we are in want of in order to satisfy the problem, are one and the same.

The above remarks merely concern the theory of the process. The actual process of calculation may of course be complicated. In the case of the clock-face it involved the determination of that fraction towards which the series $\frac{1}{12} + \frac{1}{144} + \frac{1}{1728} +$ &c. if continued indefinitely, would approximate without limit. In the present case it involves the determination of the position of that line towards which the chord PQ will similarly approach.

It is here that the so-called infinitely small quantities make their appearance. The direction of the above chord may be determined by the ratio of the two lines $QN : NP$. These lines, remember, are always finite quantities, and always determine the *chord* PQ, and not the *tangent* at P: as Q approaches P, they of course become smaller and smaller. Now what line of fixed position will QP, thus determined, ultimately *indicate*? We purposely say 'indicate' rather than 'give', because the actual PQ is supposed to be continually and gradually changing its position, whereas the line which it thus indicates is fixed. Clearly this fixed indicated line is the tangent desired. This is commonly expressed, in the language which Berkeley and many others have found so hard to follow, by saying that the ratio of QN to NP, *when both vanish*, gives the desired direction of the tangent.

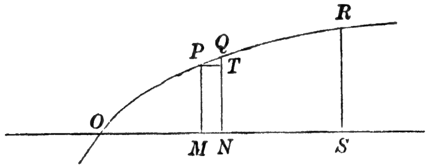
Take again the following examples. Suppose, first, we want to determine the area of the triangle ABC. We should naturally do it directly by saying that it was half the area of the rectangle $AC \,.\, CB$. But as in the case of the clock-face we may also get at the same result by a more circuitous process. We may divide

the triangle up into slips such as *PMNQ*. Then, instead of determining the triangle itself we may give the sum of all the



pieces such as *QRMN*. Now no such sum of parallelograms will ever make up the triangle; they will always be too small in aggregate area, however many they be. But they will *indicate* the area of that triangle, and this with perfect accuracy. For the area towards which such a sum of parallelograms indefinitely approaches is none other than that of the triangle we want.

In the above example we had a case where the employment of limits, and the machinery they involve, was quite unnecessary. Take now the following.



Suppose we want to determine the area of the curve *ORS*. Here we have no such simple resource as in the case of the triangle, whose area could be given at once by simple geometry. Our only plan is to conceive the area divided up into a multitude of strips such as *PMNQ*. We then begin by taking the sum of all the parallelograms such as *PTNM*. As before, this is not the result we want, and never could yield it exactly. But it will indicate that result, for the only area towards which such a sum will approach without limit is the area of the curve in question.

If this were a work on mathematics we should have to give a detailed explanation of the process. It may seem a very circuitous way, instead of determining an area directly, to determine the limit towards which *another*, and composite area,

is continuously tending. But it so happens that the latter result, in very many cases, is attainable, and the former is not.

Beginners are so apt to confound a process with a result; in the sense of supposing that when the former can never be completely carried out, and would if suspended at any intermediate point yield an erroneous result, the final or *indicated* result must also be to some small extent erroneous, that it will be worth giving one more illustration. Suppose I have to pay a sum of £1. There are a variety of ways of proceeding to assign this sum. I may do it simply and directly, by, say, two payments of 10*s.* each. But again I may do it by beginning with 10*s.*, then giving 5*s.*, then 2*s.* 6*d.*, and so on; and saying that the sum to be paid is that which is indicated by the sum of such a series continued indefinitely. Or I may adopt a quite different plan by saying that the number of pounds is that towards which the ratio of the number of heads and tails in the throws of a perfectly fair coin tends to approach. Or finally I may say that it is expressed by the ratio of the chord to the arc of a circle as these are made continually smaller.

In all these cases the same number, *unity*, is assigned; but it is assigned in very different ways. In the first case it is given directly by a simple and finite operation. In the other three cases it is indicated rather than given. We indicate it by a proposed process which from the nature of the case can never be completed, and which is such that if we trust to it as assigning the result *directly*, would be distinctly erroneous. For, since the process can never be completed, any result which it directly yields is too little or too great. But the result which it aims at, or indicates, is perfectly definite, and in each case precisely accurate. This is the only result with which we are really concerned, the process by which it is indicated being a mere scaffolding necessary for its attainment. The general method of the Differential Calculus is of this kind; and inasmuch as it deals almost exclusively with the limiting ratios of magnitudes which are supposed to diminish without limit, it partakes of the nature of the last of these three examples.

CHAPTER XXI.

EXPLANATION AND VERIFICATION.

NOTHING is more frequent, both in science and in common life, than a demand for an 'Explanation' of some fact, or class of facts, or law. The attitude towards the phenomena of nature which prompts this demand, is one of immemorial antiquity. Science, in fact, has taken the conception from common life, doing nothing more than defining it, widening its scope, and making its conditions as stringent as possible.

However far back into the past we try to project ourselves, we cannot, of course, reach a time in which a very great amount of experience had not been already reduced to order and become established and accepted. The earliest dawn of self-consciousness and enquiry presupposes ages of semi-conscious and unenquiring dependence upon the order of nature; for no life could continue to exist without such dependence. The earliest date therefore from which we can suppose ourselves to start is one in which portions, so to say, of the warp and woof of nature have already been woven together,—just sufficiently to yield a certain amount of firm texture,—but in which these are nothing more than patches, surrounded by innumerable skeins in loose but apparently inextricable confusion. Take, as a case in point, the attitude, as we may conceive it, of some unusually intelligent savage. He finds himself surrounded by myriads of isolated facts. These are quite enough to prompt the curiosity; and the questions so incessantly put by a young child seem to show that, as things now are, mere curiosity is an abundant motive for enquiry. But one would suppose that the primitive motive must have been one of a far more serious and urgent character. An isolated fact may merely arouse the inquisitiveness of a modern child, for his conduct is controlled by the care

of those who are more experienced; but to the man who has to face the consequences of his own actions such a contingency means danger, for it may at any moment result in injury or death. His position may be compared to that of some ignorant stranger who has wandered into a gigantic foundry or workshop. He can hardly touch anything without a risk of being burnt: he does not know where he can stand without the chance of being knocked down: at any moment he may be crushed by a steam-hammer, blinded by a spark, or swept away by a revolving band.

If this is at all a fair illustration of the position of man in an unfamiliar or 'unexplained' world, it is obvious what sort of desire he must experience. This is, in a word, the desire for order or interconnexion between the facts which surround him. This, I consider, points to the primary and familiar signification of explanation, or at any rate corresponds to that desired improvement in our intellectual position which we afterwards come to designate by the name of explanation. If the reader will recall what was said in an earlier chapter about the nature of Causation in its primitive and popular sense, he will see that 'explanation' almost exactly covers the same ground as what we there designated as 'uniformity' in its widest sense. By Uniformity, as there stated, we may understand any kind of order whatever, any arrangement of the things about us which enables us to anticipate without actual experience. Such order is in fact the objective counterpart of inferribility. In this sense any of the innumerable questions, put by the child or the grown person—What is that? Why does this happen?—are demands for an explanation; and for all purposes of a merely practical kind are reasonably answered by the suggestion of any rule or generalization whatever which will serve to bring the facts in question into some relation with other facts already known, or, at any rate, more familiar.

When we take this view of the embarrassment out of which the desire for explanation springs, and of the simplest means at hand for practically removing our difficulty, we see that many forms of proffered explanation which the popular mind accepts, and which the scientific mind scornfully rejects, have something to be said in their favour. Remember that the only really important requirement, in the very early stages of scientific

development, is to link the fact in question to other facts, and we shall see that few of the commonly proposed explanations fail in some measure to secure this kind of help for us.

(1) It will be well to examine one or two of these rude attempts at introducing order and arrangement, before we go on to the more precise demands of science. Take for instance, that scandal to the budding science of the nursery, when in reply to the question, Why such and such a thing has happened? we were promptly answered, 'Because it always does.' Even this statement is a help. The answer does not merely repeat the observed phenomenon; it to a certain extent generalizes what to the child seemed something new to experience. Of course the limits under which the generalization holds good are not stated here; they are barely hinted at; but some faint indication of their existence is given in any statement that such and such an event always happens. The important thing is to be reminded of the existence of a number of other occasions on which what we call the same event is found to occur.

The form in which this rudimentary explanation commonly displays itself is by the application of some already familiar word to the apparently new fact. This marks a certain step onwards,—that, say, from the standpoint of the nurse to that of the governess,—for the limits of the generalization begin now to take an outline; albeit we only get this outline in the indications of a popular word rather than by having it expressly formulated. 'Why is it difficult to walk on ice?' the spontaneous answer of the school-room or drawing-room will be, 'Because ice is slippery.' This is an explanation, though not the best we can obtain. The answer removes the experience in question from the awkward and dangerous category of *isolated* facts. The ice is thereby classed with muddy pavements, polished floors, smooth marble steps, and so forth. A slight advance has been made towards the great aim of all rational treatment of nature, by classing a number of different things according to their resemblances, and relegating the new fact to its appropriate class.

If we are right in supposing that a demand for an Explanation is primarily nothing more than one for any

sort of Uniformity which shall cover the fact in question, it stands to reason that every improvement in our conception of Uniformity will entail a more stringent recognition of the requirements demanded in an explanation. Now the standpoint commonly adopted in Inductive Logic is that which we have called the 'popular scientific' one. It is that in which we claim the existence, as antecedents to any phenomenon, of a group of invariable conditions which we call the 'cause' of the phenomenon. All that is maintained by this claim is that, on the occurrence of that group of conditions, the phenomenon will always immediately follow.

(2) The next stage therefore of Explanation, as commonly recognized, is attained by the offer of a 'cause' of the phenomenon. So far we are still dealing with facts in the concrete:—the *analysis* of the facts will occupy our attention presently. But we have made a great practical advance, for we have quite possibly put it into the power of the enquirer actually to produce the phenomenon; and have, in any case, probably put it into his power to anticipate its occurrence. Mere generalization may contain no element of foresight; but the assignment of a group of antecedents does put us into the position of being able to look a little way ahead. And if the circumstances in question are amenable to human control, then the foresight will take the form of power to secure the good and to obviate the evil. We notice a plant that is flagging on a hot summer day: next morning it stands up again fresh and green. 'Why has it revived in the morning?'—'Oh they always do.' Even this account, as we have already admitted, has a certain value, and the aspect of things is rendered slightly more orderly and rational thereby. But when I assign as a 'cause' the group of circumstances represented by the moisture and coolness of the air during the night, I have taken up a distinctly superior position. We can foresee the consequences when these antecedents recur, even though their production may be beyond our power, and this may be a valuable guide in practice. For instance, there may be a plant which I expect will die, and which I am about to throw away as useless: rain comes on to fall, and I foresee the consequent recovery. And it may happen that I can produce the particular combination which nature has afforded by the rainfall; say by putting the plant under a

shade and watering it. In many cases the dictum attributed to Bacon[1], that "knowledge is power" holds good.

(3) The above renderings of the nature of Explanation need no further comment. The subject has been abundantly discussed in an earlier stage, when we were considering the nature of Uniformity and Causation; and the reference of any assigned phenomenon to some appropriate regularity of this kind is a mere application which offers nothing really new.

But the demands of science will not stop at this stage, amply as such results may suffice for common practical necessities. We want to explain our explanation. There is nothing far-fetched or over-refined in such a claim. If explanation be the introduction of an order or regularity, it is merely relative. We have, so to say, attached our fact to certain other facts lying about it. But unless these again are similarly attached to others outside *them*, our knowledge is certainly incomplete and may prove insecure. If we could by any possibility get beyond mere phenomena we might insist on stopping only at a firm bottom; but when we are dealing with nothing more than the order of phenomena, it is just as reasonable to call for the second step as for the first. I know, say, whereabouts on a shelf a book is; this is a reference of it to neighbouring books, and is so far to the good. But it is just as reasonable to ask again what is the reference of the shelf to the bookcase, and of the bookcase to the room, and of the room to the library. Once admit the phenomenal nature of Explanation, and assume that we are untrammelled by mere considerations of practical convenience, and these further calls upon us for additional reference become inevitable. Hence it comes about that what are now commonly understood as scientific explanations generally assume, as Mill has described, one of three various forms. His account of the matter seems to me substantially correct.

(i) One of these forms of explanation consists in mere generalization. Of course the scientific man who proffers this account does not, like the nurse, simply repeat the question in a categorical and general form. He usually takes care to save his dignity by clothing his statement in a decent periphrasis. Why does this stone fall to the ground? At first hearing,

[1] Probably a popular rendering of his words "scientia et potentia in idem coincidunt."

indeed, the answers may sound very different. (*a*) Because stones always fall; (*b*) Because of the Law of Gravitation. When we look at it closer, however, there is not the slightest distinction of principle between these two replies. Each is a generalization pure and simple; without analysis, and without assignment of any group of antecedents. From the present standpoint of science nothing more than this is possible; for though there have been many speculations as to the nature of what we call gravitation, no scientific man will pretend to say that there is yet a generally accepted theory existent to account for it. Where the scientific answer here has the superiority is not shown in the fact that it goes deeper down, but that it, so to say, spreads wider out. We see this by comparing the successive stages, in respect of range of answer, that might be advanced:—'because all stones fall': 'because they are heavy': 'because of the attraction of the earth': 'because of Universal Gravitation'. The form and apparent purport of these answers is somewhat different. The first is frank and *naïf:* the second suggests that the falling bodies themselves are the agents of the motion: the third suggests that it is the earth which is the active party in the result: the fourth is strictly neutral as between the two parties. All this is insignificant, and may even be misleading. The really important point about the final answer is to be sought in the fact that the expression introduced, 'universal gravitation', is always carefully defined so as to remind us that the term applies not merely to falling stones, heavy bodies, earthly bodies, but to all material substances whatever.

Another case in point,—always confining ourselves to the existent state of knowledge; that is, with the reservation that the next generation may look at the matter very differently,—is to be found in the disposition of plants to grow upwards. All plants, as far as known, I believe, *start* upwards as regards their stems, however these may begin soon afterwards in some cases to creep. And they all equally start downwards as regards their roots, whatever direction these may subsequently take. When we enquire as to the cause of this tendency, directing our questions as before, to persons in different stages of intellectual culture, we find a much greater semblance of agreement in respect of the answers received. The scientific

man does not attempt here to interpose a technical term like 'gravitation': in fact, owing to the novelty of the enquiry he is not provided with such a term as yet. Had the particular question been raised a couple of centuries ago, the difficulty would probably have been smoothed away by the introduction of a well-selected expression, on the analogy of 'plastic form' or 'vital force'. But this resource is not available, and consequently to the semi-scientific, who are greatly influenced by the appropriate introduction of a term, it often seems in such a case as if some admission were being made as to the inferior position which we occupy[1]. It seems to them as if 'no explanation can be offered', and as if therefore we were face to face with something exceptionally mysterious.

The real prerogative of the scientific man here, I take it, is to be found as before in the wider generalization, and in the more accurate definition of the terms employed. He tells us that the property holds true of all plants; and, what is also important, he tells us what exactly is to be understood by 'upwards'. He tells us, for instance, that upward growth does not mean growth towards the zenith, or towards the maximum amount of light; but that it means growth counter to the direction of gravity, or, rather, counter to the resultant direction of gravity if we modify this direction (say) by keeping the plant in rapid rotation during the period of germination and commencing growth. All this represents a real advance, but it does not, so far, lead us to an analysis and simplification. We have only restated the problem in more general terms; taking care however, as we do so, to define the words employed with the utmost care, and to indicate the limits of the generalization.

This form of so-called explanation is not often available, or rather, justifiable. So few of the phenonema which present themselves to our notice can even plausibly be regarded as ultimate, that some degree of analysis can almost always be

[1] Some well-known experiments on this subject were made long ago by the British botanist, T. A. Knight. It appears that, in some way, gravitation is the direct agency. Knight, by an ingenious device, varied the ultimate resultant direction of gravity, by growing seeds on a rapidly revolving plane. It was found that the initial direction in which the stalk and root started showed a corresponding variation.

effected nd when this can be effected it is reasonably demanded.

(ii) This brings us to the second and far the most usual kind of explanation: the resolution of the complex into the simple, or of the special into the general. The significance of this step will best be displayed by a very simple example. Why is it difficult to walk on ice?—Justly dissatisfied with the positive answer, 'because it is slippery', and the negative answer, 'because there is almost no friction', we persist in our enquiry until some one gives us the following account:—'Because, owing to the absence of friction, there is no horizontal reaction to the impulse of the feet'. Then we feel satisfied.

This seems distinctly a case of the resolution of the complex into the simple. What we have thus done is to show that the action of walking is really built up of a continuous succession of forward and backward reactions between the surface of the ground and our feet. Omitting all the details, which could only be described in a treatise on Rigid Dynamics, we may say that in order to start ourselves in walking we require a forward push. This is produced by the reaction of the earth to the backward push of our feet. We must then stop ourselves, after each such forward advance, or at least stop the front foot. This is effected by a corresponding backward push from the ground, similarly produced. Where the ground is rough, as on a road, this reaction is easily obtained: where it is smooth, as on ice, there is difficulty; if it were absolutely smooth the reaction could not be obtained at all, and therefore on such a surface, though rest and motion could be maintained,—in fact, if once commenced, could not but be maintained,—the action of walking would be impossible. All these cases are at once suggested by such an answer as we have proposed.

In the above case we did not prominently appeal to more than one simple law or generalization. Take now the following question:—Why does a balloon rise in the air? Passing by, as before, on the ground of inadequacy, the answer, that it rises 'because the balloon is light', we demand some better account. We get the following reply. The balloon is acted on, downwards, by its own weight and by the pressure of the air on its upper surface; it is acted on, upwards, by the pressure of the air on its under surface. The latter being the greater, the

balloon must rise. This is a true explanation, for it resolves the concrete observed fact into a number of simpler and more general facts, or rather laws; but the reader should observe that it stops short,—as most explanations must do,—of ultimate analysis. What it does, as we have already observed, is to connect the phenomenon in question with others in its neighbourhood, and thus to leave it no longer in an isolated position.

If now any one asks, *why* the upward pressure exceeds the downward, we are again ready to give a reply. We say that the total weight of a balloon full of gas, including the car and its occupants, is less than that of the volume of air which that aggregate body displaces, and therefore by the laws of fluids the resultant pressure must be upward, that is, against the direction of gravity. If, still further, an explanation be demanded for the fact thus stated as regards the pressure of a displaced fluid, we are again prepared to say something. A perfect fluid,—and the air is very nearly such,—is one in which the pressure at any point is equal in every direction. Accordingly the extra upward pressure on, say, a square inch at the bottom of the balloon [1], is exactly equal to that of a column of air of an inch section and of the same height as the balloon. Add all these together, over the whole bottom of the balloon, and the resultant total is greater than that produced by the total weight of the balloon and its contents, and the balloon accordingly moves upwards. The ultimate aim of science, as at present understood in regard to material objects and the forces which act upon them, is to reduce all such explanations at last to terms of matter and motion. At present we are generally satisfied if we can analyse them, upon demand, into combinations of the three Newtonian laws of motion, and the recognized (at present) ultimate laws of gravitation, electric action, and some other similar generalizations.

(iii) There is a third class of explanation noticed by Mill, which must however be received with some reserve. He says that in certain cases what was at first regarded as an immediate

[1] That is, supposing that the balloon were, say, 30 feet high, the pressure of the air at the bottom would exceed that at the top by exactly the weight of a column of air 30 feet high. And this pressure is the same in every direction. For purposes of mere explanation we may simplify the balloon into the form of a cylinder of the same diameter throughout.

sequence between A and B, is explained when we interpolate a link, and thus substitute the two sequences A and C, and C and B, and regard each of these as immediate. That something of this kind may occasionally present itself in the area of mere popular discussion is not unlikely. But the tone of the explanation is rather too suggestive of that metaphor of 'links of a causal chain', which indicates, as has been already remarked, an attitude towards the phenomena of nature now generally avoided in science. When a 'sequence' is shown to us, that is, when there are two groups respectively, of antecedents and consequents, with an appreciable interval between them, however minute this interval may be, we know well enough that if we choose to examine more closely we can subdivide this by the interposition of other so-called links, and so on indefinitely. Nature is continuous, and it depends entirely upon the degree of minuteness to which we decide to work, and upon the existence of appropriate names for the intermediate events, whether or not we interpose any of these links. But it can comparatively seldom be the case that what had been generally regarded as an ultimate sequence,—an A followed, as was thought, directly by a B,—can be conveniently resolved into two or more such,—into A followed by C, and C followed by B.

There are of course such cases. For instance, the savage who sees that the fire of a gun is almost immediately followed by the death of an animal may possibly regard this as an immediate case of antecedent and consequent. If so, we certainly explain the fact to him when we show that the passage of the bullet through the air was the intermediate link; an effect of the gun fire, and a cause of the death. And by again interposing the expansion of the gases of explosion before the starting of the bullet, we may be said to explain the phenomenon still further. So again; if a piece of wire be pulled or pushed in the direction of its length. To rude common apprehension this seems a simple event, admitting of no analysis into successive parts. Such persons regard the wire as a rigid undivided whole, and doubtless conclude that the one end starts its movement at the same instant that the other end is pulled or pushed. But it would seem certain, on any theory of the molecular or discrete constitution of matter, that the impulse must really consist of a multitude of successive impulses

following each other at inconceivably minute intervals of time, and that therefore one end really starts whilst the other is still at rest.

In the above remarks we have been speaking of this insertion of so-called intermediate links only on the supposition that they were introduced under the express designation or justification of 'explanations'. But it need hardly be pointed out that a very large part of the work of the scientific man is devoted to this very process of subdividing what might have been taken at first as a simple whole or an immediate sequence. Thus, the spark from the Leyden Jar was once supposed to be a single event: the passage of a current from one pole to the other. It was afterwards ascertained that a series of oscillations was involved, but so contracted in time as to appear a single instantaneous event. Probably in course of time these oscillations themselves will be studied, so as to ascertain the nature of the wave involved in each of them; that is the rate at which they rise and fall. Perhaps the principal field of this filling in of the details of what once seemed immediate sequence is to be found in the department of Psychophysical investigation. Innumerable successive stages have been established in such apparently immediate data as our recognition of form and colour, the after-images we experience, and the nature of the nerve currents interpolated in what was once supposed to be a simple act of volition.

In the above remarks we have not drawn any distinction between the explanation of single 'facts', and that of 'laws'. From our present point of view there is not the slightest difference between these processes. That is, the *demand* in each case is the same, though the fact of its being raised in the second case implies a somewhat higher scientific level on the part of the questioner. It is the isolated, and therefore unfamiliar, which we want to account for. A concrete event of an altogether novel kind is an extreme case of this, and therefore every one who has reached a stage in which he can consciously observe, however young or unintelligent he may be, feels that such a thing as this needs to be fitted in with the rest of his experience. Suppose however that the event is observed to happen repeatedly. This mere repetition tends to satisfy popular curiosity; for it is a sort of generalization of the phenomenon, and therefore

renders the thing familiar, although no single occurrence of the series has in any way been better fitted in with its surroundings. Now when we make a 'law' of this generalization, and claim to have this law explained, we are really taking exactly the same step again in this second stage as we did in the first: we are asking to have the group or succession of occurrences, regarded as a whole, fitted in with other phenomena.

We may see something of this kind in the case of any new disease, or invention, or in fact of any event which attracts popular attention. The cholera, on its first occurrence in England, startled people, and possibly many a village sage could only account for its unprecedented symptoms by invoking witchcraft. They know no more about its origin or spread now than they did before; but as the event is no longer a merely isolated one it has received the best part of such explanation as it seems to them to require. When the modern physician seeks for an explanation, what he wants to do is to start from such generalization as has been already effected, and to fit this in with other phenomena. This he will hope to do in some one of the three modes indicated. If the phenomena were an ultimate law,—which in the case of a disease is of course out of the question,—all he could hope to do would be to effect a more exact generalization. But what he will naturally seek to do is to discover an antecedent, or effect an analysis.

Most of the common contrivances of life which, however ingenious at first, have now become familiar to us, stand on the same footing. The action of the common pump is a case in point. The village workman is not so perplexed about it as to ask *why* it acts, though some of his early predecessors must pretty certainly have put such a question. It always does bring up the water, and that is (quite justly) enough for his purpose; the utmost length he is likely to go is to help himself out by a word, as we are all apt to do, and to account for the water rising by the fact that 'it is sucked up', even this, it is evident, involving a certain amount of generalization. The scientific man, starting with this generalization or law, explains it by analyzing it into the component elements. He shows that the pressure upwards on the column of water in the tube is assigned by the weight of the whole column of air of the same

diameter as the tube, and the pressure downwards is assigned by the weight of water in the tube. If the former is the greater, as it will be when the length of the tube does not exceed about 32 feet, the water will ascend. So far as this explanation goes, —and like all explanations it only professes to go a certain way, —it completely answers our purpose. It analyzes the already generalized phenomenon into its proximate component elements.

In the case of most explanations it is taken for granted that the laws we introduce, either by way of analysis or of interposition, are more familiar to us than the phenomena, whether facts or laws, which we are seeking to explain. But this need not necessarily be the case. The distinction between what is, and what is not, thus familiar to us at the time, is of no serious importance: at most the recognition of it indicates a slight difference of purpose, under an identity of method, between the popular and the scientific way of regarding the process. The plain man merely wants to connect the observed fact with other facts, in order that he may be able to produce the former, or to avert or avoid it; the scientific man wants to connect it with others in order to advance himself on his path towards a knowledge of nature in general. Accordingly, nothing is popularly accepted as an explanation in the way of analysis or interposition, unless the laws thus introduced are already known and accepted. The scientific man however does not much care whether these laws are already known or not: if they are, so much the better: if not, they must become known. For speculative purposes they equally in either case help him on towards that simplification of nature and reduction of all things to the fewest generalizations, which is his one great aim.

Another way of calling attention to this distinction, is by raising the question whether the *explanation* and the *proof* of any fact or facts can ever be one and the same process? Certainly they can. If the laws into which any observed occurrence, whether individual or general, can be resolved are already fully established, then such resolution is simultaneously an explanation and a proof. Of course this runs somewhat counter to popular usage; inasmuch as before an explanation of anything is called for it is always taken for granted that the fact

is undoubted. Thus the explanation requested by Charles II. of the fact that a live fish placed in a bowl does not add to its weight, is a commonly recognized illustration of the necessity of first making sure of our facts before proceeding to account for them. On the other hand a demand for proof is always understood to imply that the facts *are* doubted. But if we slightly vary our expression so as to bring ourselves into accordance with usage, by speaking of the explanation and proof of 'an asserted fact', we can easily see that these may frequently be the same thing. Suppose, for instance, we were discussing the existence of repeated Glacial Epochs in past time. We are not absolutely certain of the fact, and, granting its truth, we have not by any means accounted for it as yet. If it could be shown, however, that such a state of things would certainly result from known astronomical data, combined with the known laws of the distribution and accumulation of heat, we should at one and the same time have proved and explained the alleged fact. It is difficult to say in which of these two senses the process would most naturally be interpreted in popular parlance.

It may be remarked that Verification or Proof must be understood in a relative sense. We can never, by such help, make absolutely sure of our result. That is, though appeal to such resources enormously increases the probability of our Induction, and may in many cases lead to what we call practical certainty, we can never reach absolute certainty in any matters of physical science. Highly unlikely as it is that two independent processes should agree in supporting the same erroneous result, such a coincidence may happen, just as a mistake in 'proving' a sum in arithmetic *may* happen to confirm a blunder in the original reckoning. The following instance (for the details of which I am indebted to a written communication from the late Dr W. B. Carpenter) seems a case in point. For a long time it was supposed that the minimum temperature of the water in the depths of the Ocean was about 39° Fahrenheit; and it was held that, whatever might be the temperature at the surface, if we measured downwards we should come to a point where this particular value was reached and the temperature remained unchanged from thence to the bottom. The fact will often be found stated in older works on Physical Geography, and was deduced from the experimental result that water at that

temperature was at its maximum density. The deduction moreover was supposed to be 'verified' by direct experiments. Thermometers were repeatedly let down to great depths, and the recorded readings seemed to justify the inference. It has since been ascertained however that both the deduction and its verification were wrong. The bottom temperature of the Ocean, over enormous areas, is very little, if at all, above freezing point. As regards the inferred conclusion, it is found that the maximum density of salt water under enormous pressure is not at 39°, but several degrees lower. And as regards the experimental verification, it was found that, where the glass of the thermometer was not broken under the enormous pressure, the bulb was slightly contracted and accordingly the mercury forced somewhat too far up the tube. That in the observed cases it should have been forced up to about 39° was a mere coincidence.

The question may be raised, in somewhat loose phraseology, 'whether everything admits of explanation?' I should begin the answer here by a reference to the analogy offered by Definition. A definition, as we saw, presupposes a general consensus as to the meaning of most of the terms in use, and presupposes also that the particular term in question is not understood: its function is to bring the latter into the domain of the intelligible by connecting it with other terms which are, so to say, in its neighbourhood, and whose meaning is undisputed. Just so any demand for an explanation presupposes a considerable body of acquired experience, in the way of law or uniformity; otherwise an explanation would be unattainable. But the demand also presupposes that the fact in question is not thus reduced to order, otherwise the explanation would be unnecessary. The function of the explanation is to bring the fact in question within the domain of the inferrible by connecting it with uniformities already known, whenever this is possible.

If this be so we see at once that in the popular sense of the term everything must probably admit of explanation. For in the first place everything admits under due restrictions (pointed out in the chapter upon Causation) of generalization. And in the second place, to the best of our belief, every fact in nature is connected with others in a way which would, with due knowledge on our part, enable us to infer one of them from the

other: in other words, to detect a uniformity of some kind. It will not need more than a word here to remind the reader that this connexion or uniformity may be of almost any kind, and have any time-reference, without failing to fulfil the popular requirement. Suppose, for instance, I see a man on a stormy night digging in a lonely field. His action strikes me as strange, but I should in common parlance have accounted for the fact, whether I connected it with (1) the past, by discovering that he had once made a bet or received a command to do so; or (2) with the future, by explaining that he was looking forward to unearthing a treasure; or (3) even by a mere generalization, as by saying that he was a man who was constantly in the habit of doing so. We should probably disguise the inadequacy of the last account, as usual, by the introduction of a phrase, as by saying that the man was mad; even though the sole evidence, and the sole outcome, of the madness was the fact in question. But the mere conversion of an isolated event into some kind of regularity would be felt to be a step towards what was needed in an explanation.

But when we speak in the more restricted scientific sense it is equally clear that everything does not admit of explanation. An analysis of the complex into the simpler does not admit of indefinite procedure. Of course a process of *interpolation*,—'explanation' in the third sense of the term,—does admit of being continued without end; but then, as we have seen, this is not regarded as a very scientific kind of explanation. Moreover it is clear that this possibility of endless interpolation corresponds to the possibility of endless subdivision of a line; it depends entirely upon ourselves how much we choose to regard as a 'link' or intermediate event. Between the touch of the trigger and the death of the bird there is an opening for the insertion of as many events and changes as we choose to distinguish and appreciate.

As regards, however, the soundest kind of explanation,—that which consists in resolution of the complex into the simple,—it is clear that we shall at last, in whatever direction we may proceed, or however many steps we may make about it, come to some widest generalization beyond which we cannot go. In regard to the phenomena of matter and motion, there is a general agreement that the three Newtonian Laws are of this

ultimate description. In regard to the law of Universal Attraction or Gravity, all we can say is that we do not at present see our way to analysing this satisfactorily into any laws which are simpler or more ultimate. It must therefore stand, provisionally, as an ultimate law; that is, as a law which, in our present state of knowledge, does not admit of explanation in any scientific sense of the term.

In speaking of Verification, the general nature of this process is too obvious and familiar to stand in need of much illustration. It is simply an appeal, direct or indirect, to experience, in order to justify or condemn our temporary conclusions.

The direct appeal consists, so to say, in simply taking a test fact and comparing the result with our anticipation: always, as we do so, bearing in mind the caution suggested by the above example of the deep-sea thermometer. Of this kind of Verification no better example can be given than the often quoted instance of Pascal's experiment on the Puy de Dôme. According to his explanation of the rise and fall in a barometric tube there ought to have been a certain fall in the column of mercury on its being taken to the summit of the mountain. He accordingly took it there, and found the fall to occur as he had expected. Similar test cases are perpetually presenting themselves to modern chemists, physicists, and other investigators.

The indirect appeal consists in our resort, not to an isolated fact, but to some generalization already attained and accepted as provisionally certain. For this purpose the physical enquirer generally has at hand a very large number of *axiomata media*, as they are frequently called after Bacon, or intermediate generalizations. If we can show that our result is in harmony with one of these axiomata we gain confirmation; if we can actually deduce it from one of them we secure proof.

As a matter of fact it is probable that these broad intermediate generalizations are just as often appealed to for the purpose of confuting some proffered explanation as for that of proving it: perhaps indeed more often so. In either case they are equally serving their purpose in aiding us in our search after truth. An illustration or two may be given. The first of these will be afforded by a well-known argument of Herbert

Spencer in his Essay on the Nebular Hypothesis (*Essays*, I. 239). In offering an explanation, amongst other things, of the rotation of the various planets round the sun, he starts, it appears, with the supposition of a non-rotating nebulous expanse whose elements mutually attract each other. He then argues that "the probabilities are infinity to one against all the respective motions" of these elements "exactly balancing each other", as measured round the sun. He concludes therefore that, in the process of contraction and condensation, the resultant planets will have a definite rotation round the sun in one direction or the other. As an argument this is shattered at once by the physical principle called the Conservation of Moment of Momentum[1]. The planets as solids cannot display the slightest amount of rotation more or less than was originally possessed by the nebula as a whole. That is, we can find no explanation here of the origin of the planetary paths.

Another and less familiar example may be taken from a speculation of the late J. A. Froude. It is merely offered here as an illustration, for his name carries no scientific authority. He observed that in the Southern Seas the Trade Winds produced a heavy and almost constant swell in one direction, and inferred that "a series of enormous waves for ever moving in one direction, over so large a portion of the earth's surface might in some degree counteract the force which is supposed to be slowly stopping the rotation of our planet" (*Oceana*, p. 75). He has foundered here on the same rock as did Spencer. It would be difficult to disprove such a theory in detail, owing to the impossibility of making a quantitative estimate of all the forces at work. But the physicist would at once point out that as the Trade Winds are not, like the tides, due directly to the Moon's influence, any action of the waves produced by them must be regarded as an internal force. The earth, with the superimposed water and air, is here our 'system', and no internal interaction can alter its rotation.

[1] The principle is that the Moment of Momentum (in popular and very inaccurate language, the total amount of rotary force) of a system of bodies cannot be altered by any but external forces. If the system display, at first, no rotation as a whole, it cannot acquire this by any mutual interaction of its constituent portions. Spencer's error was pointed out in a very trenchant review in the *British Quarterly* for October, 1873.

CHAPTER XXII.

UNIVERSAL OR PERFECT LANGUAGE.

THE intimate connexion between Language and Thought, upon which we dwelt in an early chapter, is an abundantly sufficient ground for looking to some reform in the former as likely to afford powerful help towards advance in the latter. We may conceive of reform being carried out, here as elsewhere, in either of two ways: namely by constitutional means, or by revolutionary efforts.

I. By constitutional means, in this analogical sense, may be understood such means as are conveniently provided by the machinery which we already find to hand; that is, which only require the use of existing resources in a watchful and progressive spirit in order to secure a real and steady advance. The old Logic provided, according to its light, certain aids for this purpose. *Definition*, for instance, must be considered as being one of these aids. It was not indeed proposed, under this heading, to introduce expressly any new terms; but by rendering the signification of old terms more accurate, and by extending this accurate signification as widely as possible amongst mankind, the old writers distinctly provided for such a gradual reform of language. And therefore not without reason was Definition currently assigned a place among the main functions, or *munera*, of Dialectics. Another way in which the old writers cooperated in the same direction was in their treatment of Division, and especially of the Categories. But as the aid thus furnished to the reform of language was somewhat indirect, it will be best to reserve the consideration of these topics to a later stage in this chapter.

Philosophical reformers, however, have entertained far bolder schemes than any which can be reduced to rules of Definition or of Division. What they have aimed at was nothing less than the direct, one might almost say the violent, introduction of a new and universal language.

The motives operative in the production of such schemes have been very various. The proposers have sometimes had no deeper purpose than that of just undoing the results of Babel, being mainly influenced by the practical inconveniences of the present state of things. At certain epochs these inconveniences have naturally been more acutely felt than at others, and the middle of the seventeenth century,—when some of the most elaborate and serious schemes were proposed[1],—seems to mark one such epoch. At an earlier period than this, at any rate in the Middle Ages, the powerful influence of the one dominant church, and the narrow bounds within which learning and culture were confined, must have rendered the variety of vernacular tongues comparatively harmless amongst the educated classes. There really was, to all intents and purposes a universal language for those classes. Provided any clerk was equipped, as of course he generally was, with some knowledge of Latin as well as of his own vernacular, he would feel little need for any additional medium of communication. Merchants, and the few travellers of those days who were not clerks, may not have found such a state of things the best for their particular purposes; but the bulk of those who read and who wrote books would probably find the existing arrangement perfectly satisfactory.

With the Reformation, of course, there gradually began to come about a change. The break up of the ancient church led to the necessity of a constant appeal to every man in the tongue wherein he was born. To this must be added the collateral influences which were at work at the same time: for instance, the gradual extension of literary and scientific training into strata of society which had hitherto been strangers to such

[1] It is worth noticing how nearly simultaneous were the following works, which seem mostly to have been produced in independence, if not ignorance, of their respective predecessors:—*The Universal Character*, by Cave Beck, 1657; —*Ars Signorum*, by G. Dalgarno, 1661;—*Character sive Clavis*, by J. J. Becher, 1661;—*Polygraphia nova*, by A. Kircher, said to have been published in 1663; —*De arte combinatoria*, by Leibnitz, 1666;—and the *Real Character*, by J. Wilkins, 1668.

influences; the vast expansion of commerce and foreign travel; the gradual coming towards the front, in respect of numbers, wealth and power, of nationalities which had previously been comparatively insignificant. Taking all these considerations into account, we can see reasons why ingenious and sanguine persons should begin to speculate as to whether something could not be done to aid mankind in their communications with each other.

Merely practical considerations of this description do not much concern us here. Moreover the current of events seems now tending towards a settlement of the difficulty which was far indeed from being anticipated by early speculators about a common language. If any one existent language is ever to become the universal one, Englishmen at any rate cannot entertain much doubt as to which of those now in use will obtain the mastery. Any speech which has not only already got into the foremost place as regards the number of those who use it, but also possesses the enormous advantage of complete occupation of most of the countries which still offer scope for any very great increase of population, can scarcely fail in lapse of time to become something like a general medium of intercommunication for the whole world.

If merely practical considerations of this kind were worth discussing at length here, I should be disposed to offer a suggestion, very different from that of inventing an entirely new language, to help us out until the remote contingency above indicated comes to be realized. As it is a mere digression in a work of this nature, a paragraph or two must suffice. What the suggestion amounts to is a proposal for a sort of offensive and defensive league amongst the speakers of the three languages which hold now the foremost place, by which they should agree each to speak their own and listen to the speech of the others. At present it seems to be taken for granted that when two strangers from different countries converse together they must both use the same language, whichever it may happen to be. Surely this is both unnecessary and unscientific. What ought to be aimed at is that each speaker should adhere entirely to the one which alone he knows perfectly, that is, his own, and leave it to the hearer to interpret what is said.

That there might be some slight awkwardness felt at first in such a practice is likely enough. We are so accustomed to speak and to reply in the same language, that, as experience shows, a person familiar with more than one language can hardly prevent himself from replying in that in which a question is proposed, however often we may change the vehicle of conversation. But there is surely nothing deeper than custom in this. When one is intent on the meaning rather than on the words, a frequent change of medium is not in the least disturbing. Every scholar is familiar with the experience. He may be reading a work written in English in which are notes in German and extracts from Greek and Latin, but he feels no awkwardness in this medley of tongues, and scarcely even takes account of the change of language. But a question and its answer need be no closer together than two such notes, nor closer than two successive questions; and there seems no reason therefore why they should necessarily be couched in the same language.

The prevalent practice of always speaking and answering in the same language results in a serious loss. That much of this might be escaped by the adoption of some such plan as that above suggested seems plain. This saving in fact depends on the general principle that we can understand and appreciate what we cannot perform; in other words, that we can catch the precise import of a sentence which we could not possibly have constructed for ourselves. If this were not so, we should never be able to admire the smoothness of a style, or the fitness of an expression, which surpassed what we could ourselves compose. To any but those gifted with an exceptional linguistic facility, the saving of time involved by only having to learn a foreign language up to the understanding point, instead of having to drudge on till he had reached the performing point, would be very considerable. Those who belonged to the favoured group, —say the English, French, and German,—would find themselves not indeed in possession of a single language, but in possession of what to some appreciable extent took the place of one.

Practical considerations of the above kind formed however but one of the influencing motives in the minds of those who endeavoured to introduce a universal speech. It is impossible to read some of the early speculations upon this subject without

seeing that in the minds of their authors there was a strong infusion both of Theology and of Mystic Philosophy. They were not so much endeavouring to introduce something new as to recover if possible something old. They had before their minds the notion of a one primæval language spoken in Paradise, and probably hoped that they might aid in bringing mankind backwards a step nearer to the condition of primæval innocence. They looked forward to retrieving in these later days the deplorable loss incurred at Babel.

II. Any schemes however which were mainly regulated by such motives as those above mentioned, do not strictly concern us here. What, on the other hand, men like Leibnitz and Wilkins contemplated was not primarily a Universal Language, but rather one which should become universal because it was Philosophical: its main function was not to be that of merely communicating existing ideas from man to man, but rather that of improving at the fountain head both the ideas themselves and the methods of combining and analysing them. We shall best see our way towards what was thus aimed at by commencing with a discussion of some of the obvious requirements of such a scheme if it is really to answer its high purpose. Some account will then be given of the proposals hinted at, rather than explained, by Leibnitz, and of those which Wilkins worked out with elaborate detail.

(1) For one thing then it seems clear that something more must be aimed at than a mere system of shorthand. Such schemes as those commonly so named refer entirely to some one language, and are little else than the mere substitution of certain new characters, of the most compendious form possible, for the words of that language. Shorthand makes many abbreviations, and will doubtless often substitute a single symbol for a whole phrase or sentence; but it always looks primarily to the *words* of some particular language for which it is constructed, and only indirectly to the *ideas* themselves which are so expressed. Hence it is special and not universal. The distinction in question is conveniently indicated by one of the many technical terms which have been employed by writers on the subject, viz. *Ideographik*, as the Germans write it. This term is expressly intended to mark the direct application of the proposed set of symbols to the ideas which are

more or less common to all people, rather than to the words which are peculiarly employed by some one language in order to convey those ideas. The term is contrasted, with a minuteness which reminds one of the famous homoousian and homoiousian distinction, with *Idiographik*. This latter term is intended to mark the employment of symbols specially appropriate, like those of shorthand, to one language only.

To the same general class as that of shorthand must be referred another kind of language, which however can make a far better claim to the character of being universal, namely the code used in ocean telegraphy. The necessities of commercial communication have led to a very elaborate system of symbols, —for such we may call them,—which deserves some notice in the present connexion. Any one who will turn over the volume referred to[1], will find in it a distinct attempt towards that analysis of ideas, and of consequent expression of these ideas and of their commonest combinations, which as we shall presently see is an essential characteristic of a universal language. For instance, the entire phrase, 'Have stopped shipments until we receive further orders' will be found represented as a whole by the purely arbitrary and unmeaning word *Shores*. Similarly the phrase, 'Wheat red and white, for choice white' is represented by *Viscounts*. And so on. That is, the symbols employed do not follow the specialities of our own, or any other particular language. The likeliest combinations of ideas to occur to the merchant and shipper are selected, and are represented by symbols. It is obvious that this shows in certain respects, a real advance beyond any ordinary system of shorthand. The latter would be perfectly unintelligible to a foreigner unless he knew the *details* of our language. He must be familiar with the individual words, with the grammatical inflections, with the peculiar idioms of the English language, because all these are reflected tolerably accurately in the shorthand. But for him to understand and use the telegraphic code a far less degree of acquaintance with our language would be necessary. The meaning of *Shores* and *Viscounts* could be explained to him as wholes; and when this was once done he could employ these symbols for these ideas as

[1] Bolton's *Telegraph Code*, 1871.

readily as we can. To the extent to which this holds good we have an Ideographik instead of an Idiographik.

It is obvious where such a scheme falls short of what we are in search of. Like all other inventions which have sprung up in response to purely practical needs it is strictly limited to practical requirements. Its range of recognized conceptions is very narrow: the philosopher and politician would find in it no help whatever. Moreover, never having made a definite breach with the English language, and being mainly used by English speaking people, many of its symbols are distinctly significant of single words of our own language and are meant to be so. And also, it makes none of that scientific provision which ought to be aimed at for the combination of ideas: it has no 'higher grammar', if one may so say, by which rules should be given for the building up together of the various conceptions which it employs as its units. It deserves notice however as an illustration of the way in which we may occasionally find a scheme which failed entirely when proposed by ingenious speculators, with some grand and far-reaching aim, reintroduced again by purely practical workers, and succeeding well, so far as it confines itself to the securing of merely practical ends.

(2) This guides us to a second requirement; that of the revisal and rearrangement, according to some scientific system of Categories, of the various objects of human thought. The crude idea, entertained by some early speculators, that one might by diligent comparison succeed, so to say, in collating all the dictionaries of the various languages, must be entirely rejected. It would be of no avail to try to mark the words of some one language with reference numbers corresponding to the similar words in all the other languages, so that given any one such number every speaker might name it according to his own speech. Even the words will not always be found to correspond exactly, and the sentences will constantly fail to do so. Those who entertained the notion that such exact correspondence was possible may have been misled by attending too much to the case of those very simple objects, or those abstract and determinate conceptions, which are nearly identical to all of us. So far as these are concerned intercommunication might really be as easy as, say, a commercial reckoning would be between a Frenchman and an Italian where both parties use

coins (francs and lire, centimes and centesimi) which are precisely equivalent, though they call them by different names. The familiar figures which stand for the cardinal numbers are a case in point, and thus represent, in common with the other simple mathematical conceptions, an almost unique example of what may be called exactly the same ideas prevalent almost all the world over. A mere number is read off by each individual in his own distinct language, and excites in each mind precisely the same idea. Hence, to the very limited extent thus implied, we really have a Universal (written, but not spoken) Language of the very kind contemplated.

But as regards the vast bulk of the various ideas which compose our intellectual stock, such a simple process of transfer of exact equivalents is quite out of the question. Many words of one language have really no strict equivalent in any other, the collateral and acquired associations being so different that the meanings seriously diverge. To take but an elementary example. If we cannot substitute the word 'donkey' for 'ass', or 'papa' for 'father', in any sentence of prose or poetry, without a certain change of signification, is it likely that even two languages should be found which like machines with interchangeable fittings correspond precisely, part for part, with each other?

Hence the more thoughtful of those who have undertaken the problem have recognized the necessity of a careful revision and reclassification of all the things to be named, or, what comes to the same result, of all the ideas with which we have to deal. In some of these attempts,—as, for instance, in Wilkins' *Real Character*, to be presently noticed,—this determination and arrangement of categories occupies far the greater portion of the whole volume.

(3) One more requirement is obviously necessary in order to secure all that a Universal Language should demand. This is, of course, that provision should be made for its oral as well as its visible or written communication. A set of symbols the signification of which can be at once recognized all the world over, like the cardinal numbers already referred to, is very well in its way. But we do not live by reading and writing alone; and if people are to speak with each other by word of mouth they must of course agree to attach the same sounds to the

symbols they employ. Accordingly those writers who have, like Wilkins, attempted a complete solution of the problem, have considered themselves forced to discuss at some length the number and nature of the really distinct vowel and consonant sounds utterable by our vocal organs, with the view of making the most convenient selection from amongst them. Here, again, the distinction in question has been emphasized by the introduction of a pair of correlative terms, viz. those of *Pasigraphie* and *Pasilalie*.

The value of a Universal Language which should be only adapted for written communication, as distinguished from spoken, may seem small. It is probable that some of the early writers who made such ingenious and persistent efforts to introduce such a device were greatly influenced by the example of *Chinese*. The idea seems to have been long and widely prevalent that this nation really had a number of perfectly distinct spoken languages but only one written language: that amongst them the old saying πολλαὶ μὲν θνητοῖς γλῶσσαι μία δ' ἀθανάτοισιν held good, with the substitution of speakers and readers for mortals and immortals. This idea finds its most remarkable expression in a work of the eighteenth century (Berger: *Plan zu einer allgemeinen Rede*, 1779) whose author claims to have no higher aim than "to imitate the great invention of the Chinese[1]". He considers,—what better knowledge has shown to be a rather exaggerated account,—that the various dialects within the Chinese empire were as widely divergent as the distinct languages spoken in Europe, but that the speakers of all these various dialects could freely communicate by means of their written symbols. Considering then that here was an actual working instance in point of a people in employment of one written language to convey the ideas of many perfectly different spoken languages, they naturally concluded that their only task was to improve and simplify the cumbrous arrangement of the Chinese by some better invention of a similar kind.

III. Of the many and various schemes for a Universal

[1] A number of other instances might be given from the philosophical writings of the seventeenth and eighteenth centuries, especially in Germany, showing what an impression the supposed state of things, in this respect, in China, had produced.

Language there are two which deserve more particular notice; those of Leibnitz and of Wilkins: the former from the philosophical and scientific preeminence of its author, and the latter from the ingenuity and minuteness with which it was worked out in detail.

(1) As regards this scheme of Leibnitz it would be more correct to use the hypothetical form, and to say that it would be worthy of careful study if we had it adequately set before us. Unfortunately however the hints he has communicated are so fragmentary, and occur at such very long intervals, that it is difficult to say with confidence what it was exactly that he had in view. He had probably worked out his scheme in his own mind with some minuteness; for there was no topic, amongst the many and various subjects to which he had given attention, that occupied his mind over a longer period of his life. He has told us himself how the idea first dawned upon him when, as a mere boy of fourteen or fifteen, he began to study the Categories of the Aristotelian Logic. His second published work, *De Arte Combinatoria,* when he was a youth of twenty, deals with the same idea and the best way of symbolizing it; and in one of his letters, two years only before his death, he expresses his regret that he had not a few young and helpful men to aid him in working out the details so as to complete his scheme[1].

As it is, we can say with certainty little more than this. He distinctly contemplated something much more far-reaching than a mere *dictionary.* That is, he did not propose simply to collate the various lists of terms in actual use in different languages and to indicate these by reference numbers. He mentions this as the idea of some previous writers, but rejects it as entirely insufficient. His scheme on the contrary was to be analytic. It was to be founded on a thoroughly scientific arrangement of all the possible objects of human thought. The suggestion with which, as he has told us himself, he puzzled his teachers as a boy, pointed in this direction. He had objected to the common categories, and desired an extension of them which should include not only simple notions, but also complex

[1] See the *De Arte Combinatoria* (Erdm. p. 27): the *Historia et Commendatio linguæ characteristicæ* (id. p. 162): some remarks in the *Nouveaux Essais* (id. p. 355) and a letter to M. Redmond de Montmort (id. p. 701).

notions and whole propositions. Crude as he admits his original notion to have been, it appears that such an extension of the system of Categories formed the basis of his scheme as he ever afterwards conceived it.

With these materials to start with, the next question was, how these elements of thought were to be expressed. Here again we are left in doubt. In his *Historia*, he speaks throughout of 'characteristic numbers', which seems to suggest that the successive subdivisions were to be variously lettered and numbered; so that anything might be indicated thereby, as, for instance, we might call for a book at the British Museum by its class number, (17 . 25 . 2007,) whatever it might be. Elsewhere he objects to numbers as not being sufficiently natural in their distinctiveness, and proposed geometrical lines and points; a point for unity, a group of points for the idea of plurality, &c. However this might be, *some* scheme of sensible symbols suitable for universal recognition was clearly intended. In fact the library at Hanover, where Leibnitz's MSS. are preserved, is said to contain a quantity of sheets covered with actual specimens of the character which he had adopted at one time or another.

Nor is there any more doubt, in the third place, as to the advantages contemplated by his method, than there is as to its main foundation and its mode of symbolization or communication. The method was to aid in invention and in judgment, and was not to be merely a medium of communication. There is a striking analogy between the terms in which he speaks of his invention, and those in which Bacon described his method of Induction. Each of them considered that he could put into the hands of men an instrument or "organon"—the very word used by Leibnitz,—which should not only be an enormous help to thought, but should do much towards equalizing the intellectual achievements of all thinking people. Compare, for instance, Bacon's well known analogy, between the advantage furnished by his method and those furnished by a pair of compasses when we have to draw a circle, with the following remarks of Leibnitz;—

"numeris autem characteristicis plerarumque notionum semel constitutis habebit genus humanum organi genus novum, plus multo mentis potentiam aucturum, quam vitra optica oculos

juverunt... Nec unquam acus magnetica plus commodi navigantibus attulit, quam hæc cynosura experimentorum mare tranantibus feret".

The following passages will show how subordinate he considered the mere process of communication to be in comparison with the higher functions of judging, reasoning, and discovering, which were to be aided by his method. He tells Montmort that if he were younger he should still hope "to produce a *specieuse générale*—[one of the names employed to denote his symbolic scheme]—in which all the truths of reason should be reduced to a sort of classification. This might be at the same time a kind of Universal Language or writing". And again, in the *Initia scientiæ generalis*, "whereby, when controversies arise, there would be no more occasion for disputes between two philosophers than between accountants. It would suffice for them to take pen in hand, to sit down to their tables, and to say to each other, let us calculate". It is obvious that the same motive was at work upon him when he sketched out some symbolic developments of Common Logic. For instance, in his *Non inelegans specimen demonstrandi in abstractis*, he gives rules, couched in algebraic notation, for combining by addition the two notions A and B, and thus building up a complex notion which we may indicate by $A + B$. Such a combination, thus symbolically expressed, was to play the part of a universal sign for the purpose in hand, but it was primarily meant to advance thought rather than to communicate it.

As we are not concerned with the history of opinion on this subject, and still less with that of Leibnitz in particular, it would be a waste of time to speculate on the probable outlines of his scheme. Where a definite conception,—that of a Universal Language,—is before us, it will serve our purpose better to consider what has been done by some person, however inferior in capacity to Leibnitz, who has actually worked out his scheme in all its detail.

(2) Such an instance is admirably furnished by Wilkins, in his work, so well known by name, *A Real Character and Philosophical Language*[1]. The general conception of this work

[1] The work was published by order of the Royal Society in 1668. Wilkins was at that time dean of Ripon, and afterwards bishop of Chester. He was one

is similar to that of a number of others on the same subject; his scheme starting like that of Leibnitz, from the doctrine of the Categories. That is, he commences with a systematic division or classification of all the possible objects of apprehension and communication. The language in which he speaks of this process is nearly identical with that in which Mill introduces the Categories in his Logic, as a substitute for those of Aristotle. "The first thing to be considered and enquired into is, concerning a just enumeration and description of such things or notions as are to have marks or names assigned to them.... The principal design aimed at in these tables is to give a sufficient enumeration of all such things and notions as are to have names assigned to them, and withal so to combine these as to their order that the place of everything may contribute to a description of the nature of it."

Wilkins expressly contrasts this method with that which had been adopted by some of his predecessors, who had started with a classification of the *words* in some language rather than of the objects to which the words refer. The labour expended in drawing up his list of categories is very great. He assigns forty of these primary divisions,—showing in this respect a much more adequate appreciation of the magnitude of his task than was possessed by some others who wrote long after him,—and even these forty only include what he calls *integrals*; a large number of particles being put aside for separate treatment. The elaboration of these categories with their successive divisions occupies 300 pages. The portions of the work which refer to Natural History were composed for him by learned friends; that, for instance, which deals with plants being said to be the work of the celebrated Ray. The arrangement made in this latter case was doubtless suggested by the special object which Wilkins had in view, as the grouping of the genera would have been almost grotesque on any ordinary principles of scientific classification.

These forty primary categories are then divided into 'dif-

of the original founders of the Royal Society; and was the author of several ingenious mathematical and physical works. His work on Language, it may be remarked, narrowly escaped destruction in the great fire of 1666. Much of the MS., and all the already printed copies, with two exceptions, did indeed perish, and the author lost two years in reproducing his work.

ferences', and these again into 'species'. This triple subdivision comprises the bulk of the subject matter; but as it deals, so far, mainly with material considerations there still remain a large number of modifications, grammatical and otherwise, which have to be taken into account. The former Wilkins calls by the name of 'Universal Philosophy', the latter by that of 'Philosophical Grammar'.

The next step of course is to devise a set of symbols or characters to denote these classes. He lays down, very reasonably, the main principles which ought to guide us in our selection; namely that the characters should be legible, easy to write, readily distinguishable, and so forth. It is also maintained that they should, as far as is compatible with their essentially arbitrary character, have some signification or suggest some analogy: that is, that the same sort of meaning in different directions should have the same sort of sign; that slight modifications of this meaning should be indicated by slight alterations of this sign, and so on. The symbols he selected resemble in their general character those of some modern systems of shorthand, and, as far as one can judge from their appearance, are capable of easy and rapid use[1].

The working of this scheme is as follows. Each of the forty main divisions has a certain symbol attached to it; whilst the successive determinations or specializations, required in order to narrow the general idea down to the specific one which is supposed to be before us, are marked by their appropriate symbols. One of his own examples, that of the word or notion 'father', will serve to explain this. He says of it "This next character, being of a bigger proportion, must therefore represent some integral notion. The genus of it is appointed to signify *œconomical relation*. And whereas the transverse line at the end towards the left hand hath an affix, making an acute angle with the upper side of the line, therefore doth it refer to the first Difference of that Genus, which according to the tables is *Relation of Consanguinity*. And there being an affix making a right angle at the other end of the same line, therefore doth it

[1] His notation was probably suggested by some system of shorthand in actual use. It is worth noting that he speaks of the use of shorthand as being at that time almost confined to England, and as being a matter of interest and surprise to foreigners.

signify the second species under this Difference, viz. *direct ascending*, by which the notion of parent is defined". And similarly with any other of the integral notions contained in the Category.

The nature of the above process is more clearly described in a later, and very inferior production to that of Wilkins, published in 1797 under the name of *Pasigraphie*. It was anonymous, but issued from a so-called *Bureau de Pasigraphie* at Paris: it appeared simultaneously in French and German[1]. The author was far from adopting Bacon's apophthegm about the vastly superior subtlety of nature over art, for he attempts to construct his scheme with no more than twelve arbitrary symbols. Of these symbols he contemplates only combinations involving three, four, and five, respectively. The simplest of these is regarded as sufficiently comprehensive to represent the common wants of everyday life; the second class is intended mainly for merchants and men of business; whilst the third will suffice to grapple with the mysteries of philosophy. A couple of examples will amply suffice. Take, for instance, the word *Investigation*. This is considered to demand a combination of five symbols. The first directs us to the heading which treats of 'perceptive and intelligent man': the second to a column headed 'mind or spirit of man': a third to the powers of thinking, and so on. The fifth such determination, it is thought, would be sufficiently precise, and would, in any complete pasigraphical dictionary, land us definitely in the word 'investigation'. In the same way he even attempts to run down and identify proper names. Thus Ratisbon, or Regensburg, is to be identified by four steps. The first lands us in 'parts of the world'; the next takes us on to those which are 'devoted to trade or business'; the third to those in Germany.

It needs no showing that such a process as this,—which strongly reminds us of the children's game of guessing an object thought of, after three or four questions about it have been answered,—cannot possibly conduct us beyond generalities. Stating the problem in the technical language of Logic, such a procedure serves us moderately well through the successive stages of a Category till we come down to an *infima species*.

[1] It is described as being the production of J... von M..., an ex-officer of some German State.

Then come the so-called individual distinctions, and there such a scheme leaves us in the lurch.

So far we have been concerned only with the written symbols. But the full title of Wilkins' book is, "a Real Character and a Philosophical Language". That is, it was intended to be a spoken language as well. To carry out this design Wilkins enters into some enquiry as to the number and nature of the really distinct sounds utterable by the human organs of speech. The adjustment of the selected *sounds*, corresponding to the various subdivisions of nameable things, is closely analogous to that which was adopted in the selection of *symbols*. He chooses forty of the most simple and distinct syllables for the genera. The 'differences' by which the genera are subdivided are marked by a selected set of consonants placed after these syllables; whilst the 'species' which give the final determination are added on as a second syllable, thus making all his significant spoken words dissyllables.

The above brief indication will serve to show the nature of Wilkins' attempt; and, granting that it was desirable to undertake the work, there can be no denying that he has set about its performance in a complete and philosophical manner. He has moreover given actual illustrations of its execution. For instance, the Creed and the Lord's Prayer are displayed in full; and this both in the written character, and phonetically, in order to show how the characters should be pronounced. Comparing his own scheme with the Latin language, he dwells upon the many defects which the latter displays, in common with every other known language, in respect of redundancy, deficiency and ambiguity. As regards the one all-important question of feasibility, that is, whether such a purely artificial language could ever be practically put into operation, he entertains no doubt whatever. Indeed he estimates that its acquisition would demand about one-fortieth part of the time and labour involved in acquiring the same familiarity with Latin. Assuming that this requires about forty months, he considers that his own 'Character' could be mastered by a person of ordinary diligence in one month. Those who have gone through the labour of acquiring a knowledge of common shorthand, where no new language has to be learnt and where we have only to fit in a new set of symbols to the words of our

native speech, will regard this estimate as rather sanguine. As regards the comprehensiveness of his work it ought in fairness to be stated that the latter part of it is devoted to a dictionary of English words, each of which is duly assigned its place in the system of Categories. As this dictionary must contain, on a rough estimate, some 13,000 words; and as every one of these is determined by the triple assignment of genus, difference, and species, and is therefore communicable by its written character and its spoken sound; it must be admitted that the 'Real Character and Philosophical Language' does at any rate perform its undertaking to assign a convenient artificial word for all the principal objects of human thought.

It may be remarked that the sanguine expectations of Wilkins, as to the ease and rapidity with which the new language may be acquired, are common to most of these inventors; as might indeed have been expected. In fact he is far surpassed here by some of his successors. Berger, for instance (*Plan zu einer allgemeinen Rede,* 1779) suggests that the authorities should compel all innkeepers, in town and country, to learn his artificial language for the convenience of their guests. He reckons that his spoken language could be acquired by an average person in eight days, and that then *one* more day would suffice for the acquisition of the written character. Cave Beck however (*Universal Character,* 1657) had gone further still. He claimed for his invention that "the practice thereof may be attained in two hours' space".

Any attempt at a history of these inventions would have to give some account of the different symbols or characters which have been employed. As may be supposed, these are very various; no two authors apparently having made any attempt to work in concert. The majority have proposed entirely new symbols, choosing these by the sole consideration of economy of time and trouble in writing them. Some have employed characters bearing a generic resemblance to those of modern shorthand (e.g. Wilkins; Kalmar, *Præcepta grammatica,* 1772; Berger). Others have employed ordinary numerical figures. This was suggested by Leibnitz, and was put in practice by Beck, and Solbrigius (*Scriptura œcumenica,* 1723). Others, again, have employed ordinary letters and words, putting their own novel interpretation upon these. This was the plan

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adopted by Dalgarno (*Ars signorum,* 1661). Of these various plans the first certainly seems the best. If we are to go to the expense of a new language it is quite worth paying the slight extra cost of a new character for it; since our existing figures and letters, having come down from a remote antiquity, seek anything but clearness and simplicity as their primary aim.

I have discussed these schemes with what some readers may think needless minuteness. But there are several reasons for giving careful attention to them. For one thing any project in which such a man as Leibnitz earnestly believed, and to which he directed intermittent attention through sixty years of his life, deserves respectful consideration. To this must be added the practical testimony afforded by the fact that scores of men of the calibre of Wilkins have undergone the labour not merely of projecting schemes of this sort, but of working them out with all minuteness of detail[1].

But our main justification for this discussion is an intrinsic one. It seems to me that a very instructive parallel may be drawn between the state of things in the intellectual world out of which the demand for a new and universal language has arisen, and the state of things in the social world out of which the demand for a new social order has arisen. In each case the existent arrangement is the almost chaotic result of a long and complicated process of causation; it produces a mischievous and deplorable waste of resources: it has been for centuries attacked on the ground of its essential folly and injustice: and one elaborate scheme after another has been proposed by way of remedy. Of course it would be absurd to compare the actual aggregate of human suffering entailed by the present diversity of speech with that which results from great inequality of wealth, but it would be equally absurd to deny that the former is an evil.

[1] I had no conception, before coming to look into the matter historically, how numerous these attempts have been. Since the time of Leibnitz there has seldom been a decade without some new publication of the kind. Passing over all mere criticisms and notices in journals and other works, I soon compiled a list of between thirty and forty of what appear to be substantive works. If one may judge of the remainder, by the samples I have had an opportunity of consulting, the majority of these proposals must be the production of men who have devoted much labour to their task, and who are imbued with marvellous hopefulness as to the ultimate success of their schemes.

The analogy however to which I wish to direct attention here consists rather in the means towards which we may look for a remedy. Each of the successive systematisers has so far completely failed to convince any one but himself, and possibly a small body of immediate followers, that the particular scheme he has advocated would answer the purpose. All experience shows that a change of such a magnitude as is here contemplated cannot possibly be carried through; and that, if it could, the loss,—what may be called the 'capitalized loss',—of the process of change would probably far outweigh the resultant gain. But it seems none the less true that the natural evolution of events is drifting us on in the same general direction as that in which the proposers of sudden and violent change would have transported us at a bound. As we are not dealing with Political Economy here it would be out of place to enumerate the many innovations which are being almost imperceptibly introduced in the direction of Socialism. But this chapter may be concluded by pointing out some of the ways in which we are gradually realizing the various advantages which the framers of Universal Languages have striven to secure. Some of these were indicated at the commencement of this chapter, and will therefore need but very slight notice.

For one thing, what we now treat under the head of Classification, including what we aim at under the name of a Natural as opposed to an Artificial system, covers in reality a very large part of what the more scientific inventors of New Languages were striving after. The reference, above made, to the naturalist Ray will remind the reader of this coincidence of aim. In fact if we remove from Wilkins' work what we should now describe simply as Classification, nearly half of it would be abolished. The innovators, in fact, were perfectly right in the ideal they had before their minds here. From Leibnitz onwards they seem, to my thinking, to have been largely influenced by the feeling that a protest should be made against the artificial and narrow scheme of the recognized Categories of the Scholastic Logic. They contemplated Classification as a means towards improved intercourse and communication. We now see that it subserves many other ends than this, but we thoroughly agree with them in the desirability of securing what they aimed at.

So far we have been concerned with the basis of such a

proposed language. As regards the actual medium of communication there seems little to add to what has been said already. The irresistible course of events is steadily increasing the relative importance, as a means of communication, of one language out of the multitude still existent. The matter, in fact, is being simply put to the vote by the population of the world; and the resolution is,—we will not say that English shall be the Universal Language,—but, that it shall indefinitely tend towards becoming a supplementary means of communication. This concerns the language of life in general. Meanwhile, as concerns subordinate departments of life, one class of communications after another is tending to the adoption of abbreviated symbols or conventional and artificially framed words for conveying widely recognized conceptions. Thus the notation of chemistry *is* a language,—a chemist's language,—universally employed all the world over; as was long ago noticed by Leibnitz in the parallel case of mathematical notation. So in Electricity, the units which are now being introduced for universal scientific use are most certainly the elements of such a language, for the same word is employed by all to signify the same thing or notion. So too there is a growing tendency to reduce the redundancy of distinct proper names in use, by employing the same name for a place or person in all languages, instead of modifying it into harmony with the pronunciation or the whim of each distinct nationality. We should now hardly find four such different words as,—Venice, Venise, Venedig, and Venezia,—for a town or village that had only recently come to be known outside its own country. All this, of course, is but a trifling matter, but it marks a drift towards economy in respect of language. It is one little addition to the stock of really common speech. Again, in the Oceanic Telegraph Code, mentioned above, we have an abbreviated language of the description, within its own very restricted sphere, contemplated by Leibnitz, Wilkins, and others. I do not know to what extent it is used by merchants of various nationalities; but there is nothing more than such general acceptance required in order to render it, over the range of topics which it covers, a *Spécieuse générale*, or *Real Character*. It is in such humble commencements, and in such isolated departments, that we may perhaps look for the only

realization we are ever likely to see of the efforts of so many sanguine and laborious inventors of past times. It is a case of "bit by bit reform", but pending the indefinitely remote period at which the now leading language shall have acquired universal acceptance, there is plenty of time for reform of this description to effect very great results.

This chapter was written before the recent revival of interest in schemes of Universal Language which has been owing to the publication of *Volapük*, and, since then, to the more serious effort, *Esperanto.* Whether this latest attempt, or any successor to which it may yield place, will have better success than those which have preceded, time will show. Those who realize how numerous these attempts have been during the last two centuries, and with what zeal and confidence they have been supported, may be excused if they feel considerable doubts upon the subject. Two things seem quite certain. In the first place the general introduction of a supplementary means of communication would be an immense convenience to the intelligent tourist, traveller, and merchant. But, in the second place, it seems equally clear that such a means of communication can never approach to the functions of one of those national languages in which thousands of fellow-citizens have thought and felt in common from their earliest conscious hour; nor can it hope to possess those permanent masterpieces of expression which serve as a common appeal, and help to set a standard for our guidance.

In any case such attempts as these do not seem to enter into the logician's special province. They do not raise those interesting logical and philosophical topics which, for our purposes, give most of their value to such schemes as those of Leibnitz and Wilkins.

CHAPTER XXIII.

EXTENSIONS OF OUR GENERAL POWERS OF OBSERVATION.

IN a former chapter some indication was given of the nature of mere Observation, as distinguished from Inference. Apart from the particular difficulty there discussed, namely that of determining the point at which one of these processes may be considered to end and the other to commence, there is not much to be said upon the subject; regard being had to the limits within which this treatise is confined. The Psychological and Metaphysical difficulties, which underlie any theory of Observation, have to be passed by on the one side, as taking up the enquiry at too early a stage for us here. And similarly the mechanical devices, by which our powers of observation are improved and extended, have to be passed by on another side, as carrying the enquiry too far into the region of the practical. The one class of considerations is, so to say, too central and the other too peripheral to fall properly within the scope of a treatise on Logic.

There is, however, one general enquiry connected with the subject of Observation which deserves some discussion here, if only for the reason that it does not seem to have been hitherto claimed by writers either on Philosophy, Psychology, or Applied Mechanics and Instrument making. The questions here contemplated are such as these: What, in general terms, is meant by the expression "improving our powers of Observation"? Over what range, as regards varieties of observation, can such improvement be actually or conceivably effected? To what point, as regards refinement, can this improvement be carried out? The enquiry before us might be briefly expressed in language which, taken by itself, would certainly require some

explanation, by asking, What is the nature and what the limits of our control over space and time?

We start here, as always in Material Logic, from the dual standpoint; namely from that which postulates an observer and an object, the former endowed with various faculties of sense, the latter possessing various attributes. This seems to suggest three different ways in which we might conceivably improve our powers of observation; for (1) we might simply endeavour to get nearer to the object in question, so that the senses might be put to less strain in cognizing it; (2) we might endeavour to enlarge the object, or in some other way to intensify its operation upon the senses; or (3) we might confine the improvement to our own sensitive powers, by endeavouring to make these more delicate and refined. We will examine successively what can be done in each of these directions.

(I) In regard to the first of these points we are apt to forget in what an overwhelming number of cases our logical inferences are merely a substitute for simple and direct observation; a substitute to which we are compelled to resort from inability to approach sufficiently closely to the object in question. So great is our triumph, and so complete sometimes our success, in finding ingenious substitutes, by processes of reasoning, for what might conceivably be attained by a mere change of position, that we come to overlook how almost ridiculously simple would be the end at which we aim, if only our faculties of locomotion were less restricted than they are. There are myriads of facts about which, if any doubt is felt, it is dispelled almost at once by some one just going to look at the objects; there are myriads of other facts, in all essential points often just as simple, which because unfortunately we are unable to go and look at the objects, task all the powers of thought of our profoundest philosophers, and the most exquisite skill of our instrument makers, in the attempt to observe them indirectly. If we want to know how hot it is at the Antipodes we go there ourselves, or send someone else there, with a thermometer. But if we want to know how hot it is half way there, namely at the centre of the earth, or whether the centre is solid or liquid, we are led into the most intricate questions of physics and mathematics, through which our best authorities have not yet succeeded in finding their way with any certainty.

Similar considerations apply also to the case of *time.* Very many of the facts which the ordinary historian works out by elaborate comparison of records, and by inferences from these, are such as he could settle at once to his satisfaction, if only he could just step back into the time in question. The witness from whom the records have come may doubtless have been a direct observer, but we do not know that we can always trust him, and we know that we cannot cross-examine him. A few hours spent in personal observation on the spot by a critical historian himself, would sometimes be worth a whole volume of records compiled by contemporary witnesses. This must be specially the case when we are concerned rather with general tendencies and dispositions than with specific facts. People have disputed for instance, and will continue to dispute, whether and to what extent our age is more moral than former ages. What a light we should gain upon this point if only some London police magistrate, some doctor in general practice, or some shrewd man about town, could go with proper introductions into some other century, and live for a few months amongst its inhabitants.

Of course many of the past facts which we want to determine rest upon no personal testimony. We cannot appeal to the witness, because he was never present. And yet the facts themselves may be of just the same kind as those every-day phenomena in the estimate of which any one ordinary person is about as good a judge as any other. Such enquiries as whether the earth was once fluid, and whether the glacial period prevailed as far as the south of Europe, are not in themselves harder to answer than the questions whether the lava from Krakatoa was fluid, or whether there is an open sea in winter on the north coast of Iceland. So far as any difficulties in the actual phenomena are concerned they could be settled at once by any witness as good as an ordinary trustworthy skipper. He would merely have to describe what he had seen and felt in one case, as in the other, and the matter would be set at rest at once.

We can readily see therefore how important would be a more complete control over space and time,—as, for want of some better compendious expression, I have ventured to call it, —for nearly all purposes in which inference is concerned. Any such control, if really complete, would at once supersede the

need for much of our present calculation: for why carry on in the study a tedious and circuitous process, when the direct process for which this is merely a substitute, is itself within our power? And, as regards the cases in which we might still have to resort to inference, our position would be immensely strengthened. The wider the basis of observations from which we start, the better grounded as a rule are our conclusions, and the more convenient the processes of arriving at them. Every extension of power therefore which we could gain over these two all pervading conditions of things would diminish the sphere, and lighten the task, of inference.

Before looking closer into details let us just enquire for a moment, what it is that we desire: in other words, what are the requirements for that extended power of observation which, if fully complete, would render most of our inferences superfluous; and which in proportion as it approaches completeness so powerfully aids our inferences? Briefly the result may be summed up by saying that what we want is free power to move about at will, or else,—the alternative comes to the same thing, since all motion and position are relative,—to move the observed object in any way we please. Obvious as this condition is, few people realize, until their attention is expressly directed to it, how very nearly the distinction between Observation and Inference corresponds to the difference between the conditions of things in respect of which we do possess this power and of those in respect of which we do not.

Take a trivial example for illustration. I am inspecting some small object, say a penknife of unfamiliar construction, and I want clearly to understand its mechanism, size, and shape. How is it that I am able to do this so completely and accurately? Mainly because I am able to turn it about at will, so as to present any face toward me, and to put it at any required distance from the eye. By running the eye repeatedly over the main outlines, and by recurrence to characteristics already observed so as to bring them into connexion with other characteristics, we succeed in mentally building up the various parts into one connected whole. We then consider that we have taken the entire object in, or that we understand it.

Such an example as this represents the extreme in one direction; that, namely, in which owing to our almost entire

control over our relative position towards the object in question, Inference is almost entirely superseded by direct Observation. At the opposite pole lies that class of cases in which Observation gives place entirely to Inference: for instance a planet which no one has seen, but which the astronomer has inferred from its effects on the neighbouring planets; or any conclusions which we may draw as to the constitution and condition of the interior mass of the earth, or about events which are now remote in time.

But between these two extremes lies a large field over which mechanical aids can help us. If we are not able directly to alter our relative position of observation, the next best thing is to find some device by which we can do this indirectly, by attaining the same result as would be secured by such a supposed alteration. A large number of instruments are solely directed to this purpose, and may range from the simplest contrivance up to the most costly and elaborate inventions of mechanical skill. When the penknife lay before me, and I wanted to observe the under side, I just turned it over. But when the dentist wants to observe the back side of a tooth, or the surgeon to examine the state of the throat farther down than can be directly seen, they have to help themselves with instruments in the shape of duly constructed mirrors. These are nothing but simple substitutes for that direct observation which we so easily obtain in the case of the penknife. The telescope, again, is for all its ordinary purposes obviously nothing but a substitute for moving nearer to the object. If we could go up to the moon as we do to the great pyramid, we should want nothing better, so far as mere sight is concerned. As things are, we have to content ourselves with finding some instrument which puts us exactly into the same position as we should occupy if we could advance part of the way towards the moon. We have succeeded, up to the present, in thus getting presumably within about 100 miles of it.

If we compare our position in these respects with that in which we find ourselves placed as regards *time*, we see at once what a difference there is between the two cases. Suppose we are examining some short event in which the *succession* of phenomena is all that we want to attend to; just as, in the case of the penknife, we were confining our attention to simul-

taneous phenomena which were supposed to undergo no change. What we should really like to do here is to be able to stop the succession, that is, to arrest the stream of time, at any desired point; to retain it there as long as we please; and to recall the past again and again at will, in order to bring what is now lapsed into notice again and to compare it immediately with something else which was separated from it by a certain interval. If we could only succeed in doing this we should really possess the same degree of command, for purposes of direct and simple observation, over any object,—that is, over what we commonly call an 'event',—in time, as we now possess over any convenient object in space.

What a gain for instance it would be for any student of embryology if he could put his growing object under a microscope, and then shift it a few minutes or hours forwards or backwards, or keep it arrested for any desired length of time. Compare the position of a man who has got an intricate argument in writing before him with that of one who can merely listen to the statements as they are once repeated to him in order, and we see at once what is the difference between our capacities of observation as they are and as they would be in the impossible circumstances suggested. What we should really wish to be in possession of, in fact, would be some kind of microscope with a double set of stage screws: one set adapted for moving the stage about as is now done in respect of space, and the other to move it about in a similar way in respect of time. Many an intricate question which now puzzles physiologists might not improbably be thus cleared up. Our gain of power would be similar to that of some explorer of a labyrinth who should receive the permission, hitherto denied him, of retracing his path at will, instead of being obliged to move continually forwards.

A single illustration will show what is meant by this suggestion, and how great would be the consequent gain. Dr W. H. Dallinger, when investigating the growth and reproduction of certain microscopic organisms, found it necessary to keep them continuously under observation. In fact, he and a fellow naturalist kept up the observation, alternately but uninterruptedly, for several weeks. That is, if they wished to make sure that no stage of growth had been omitted or

misinterpreted, they had to keep their eyes on the object much in the same way as we watch a cork floating down a rapid stream, knowing that if we once let it out of our sight we may never recover it again. But of course, in so doing, they could only, like ourselves in the case in question, follow the cork *down* the stream. Conceive what a gain would be implied in the capacity of attaining to similar uninterrupted and minute observation backwards, so as to track out an earliest origin as we can now to some extent track out a final development.

Physical speculators have sometimes indulged in the invention of fanciful modes of attaining some equivalent of such a power as this. Since light travels with finite velocity, we are at liberty to conceive an object moving so fast as actually to outstrip the rays. Suppose a human eye receding from our solar system with a velocity greater than that of light, and occasionally pausing for a moment so as to permit the rays from the objects which it was leaving behind to overtake it and record their impressions. We should then invert, so far as that eye was concerned, the course of events; and this would be, so far as all visual considerations were concerned, equivalent to that regression into past time which we are supposed to desire. Sound waves, of course, travel with far less rapidity, and therefore, if we are at liberty to recognize a distinction between the greater and less absurdity of speculations which lie quite outside of the possible, we may say that the corresponding supposition is not so outrageous in this case. Projectiles from our great guns actually do outstrip the sound of their discharge. If therefore one of these were gifted with an ear, and the requisite degree of consciousness, the first thing which it might hear concerning itself when it had come to rest, would be the word of command to fire it and the report of the explosion which discharged it.

Our practical substitute, or as nearly such as we can secure, for the power thus denied us, is to be found in devices which give one of their principal characteristics to what are called the Comparative Sciences. Suppose, to revert again to our penknife, that the object is behind the glass of a shopfront so that we cannot reach or handle it. The next best thing to being able to turn it about in all directions would be to obtain a sight of a number of specimens lying in different positions: granted, of

course, that we felt justified in assuming that they were all of an exactly similar construction. For the purposes of direct observation this would be a very near equivalent to the power of handling the objects.

There are a number of cases in the Biological sciences,—where, as we know, the sort of uniformities called natural kinds are most to be found,—in which we are able to resort largely to this device. Suppose, for instance, we are examining the process of germination of a seed. We find it the best plan to grow a large number and to select from amongst these several for examination, thus securing that there shall be some of them in all the successive stages of development. When we want to go backwards, that is, to revert to a stage just previous to the one which at the instant is under our observation, since we cannot do this with the *same* specimen, we take another just like it but a little way behind in respect of its development, and examine this instead. This process of course is familiar enough to every one, but what is not always realized is that it is a mere substitute,—so far as direct observation is concerned,—for that freedom of mobility relatively to the object, in respect of time, which we so fully possess in many cases in respect of space.

It need hardly be pointed out how defective any such substitute as this must be. Even when we can multiply or repeat at will objects, of whose exact similarity with the one which we desire to examine we are well assured, one serious defect makes itself apparent. The observation is discontinuous, not continuous. When we are going, so to say, the right way forwards, we can keep the object uninterruptedly in view, as with the cork floating down the stream; but when we attempt by such a device as this to obtain a sort of backward track, we can only succeed in doing so at isolated points; we must catch sight of different corks when and where we can; and therefore however many of these we may choose for our purpose,—and the more we choose, the greater the labour and uncertainty as to their substantial identity,—we can never entirely obviate the chance of missing some important stage. And even such a device as this is only of very partial applicability, since many events cannot be repeated at our choice but have to be waited for in due course of nature.

A solar total eclipse is a conspicuous case in point. All that

the observers on one of these expeditions have to do is simply to observe; the subsequent calculations being mostly carried out at home and probably by other persons. They have nothing to do but to say what they see, and the whole event with which they are concerned does not generally last more than two or three minutes. Were what has been called above a "free handling" of the event possible: were we able to pause at the critical moments, to recall the past for fresh determination and comparison, to re-examine at its origin some characteristic which has unexpectedly displayed new importance, a single really good observer might probably, in an hour, settle questions which still divide and perplex the astronomers. As it is we have to submit to an expenditure of many thousands of pounds, to employ in the aggregate many years of skilled labour, and to wait no one knows how long to have the problem solved.

(II) Another requirement, or at least another conceivable postulate which would enormously add to our powers of direct observation, may be described as that of being able to enlarge or diminish the actual size of the object under inspection. It need not be said that anything of this kind is absolutely out of the question in most cases, and is generally only possible to an infinitesimally small extent in others. But as the charge of absurdity as to matters of fact in respect of his examples and illustrations lies very lightly on the conscience of the logician, we may reasonably spend a minute or two in pointing out how much better we should be situated in respect of our observations if things were other than they are. There is use in this, if only to make us better understand the nature of the advantage in the few cases in which we enjoy it, and to see the rationale of the substitutes which we have to employ.

As a mere hypothesis for illustrative purposes, enlargements and diminutions of physical objects are not unfrequently proposed, and this to an almost unlimited extent. Newton is said to have suggested that if the earth were compressed until its component particles were in contact it would not be more than a few inches in diameter. Lord Kelvin has hinted that if a drop of water were enlarged to the size of the earth its constituent molecules might be somewhere about the size of a cricket-ball. What we can actually effect in either way, on most solids, is probably measured by a fractional percentage.

Our nearest substitute to such hypothetical power is found, not in actually enlarging a very small body but in aggregating it with a multitude of others precisely similar to it; if such can be found. If, for example, a chemist have submitted to him a single drop of water, and be asked to say how much solid matter is contained in it, it is absolutely impossible for him by any known means to give an estimate. But let him have a large supply, and by a simple process of evaporation it will become a matter of very little more than direct observation to give a tolerably accurate determination as regards the quantity. If we are shown two glasses of water, one from the sea and one from the Lake of Geneva, no one can detect any difference in their colour. But let us have enough of each in vessels side by side, and any eye could detect the degree and nature of the contrast.

In the above instance we were able to effect our purpose, because we were dealing with a homogeneous substance which was supposed to exist in great abundance and in comparatively uniform condition. We were in the same position as that of a man calling for a map on any scale he wishes, where what we may describe as the 'same thing' can be obtained as large or as small as he likes, according to the purpose he has in view. But instances of this kind are exceedingly rare. And the same may be said of the converse case—that of diminishing the object we are dealing with,—for this must also be taken account of. Except in the very rare instances, just mentioned, it will generally be found that in any attempt to reduce a phenomenon or an object to much smaller dimensions we have made a serious change in its condition. For example, we may illustrate the phenomena of thunder and lightning with an electrical machine, and if we knew for certain that the one was only a miniature, with every essential preserved, of the other, this would be a great help. But then this, to the early observers at any rate, was just the point to be proved. And it was quite certain in any case that the differences between the phenomena, besides those dependent upon mere magnitude, were many and important. Little therefore need be said on this head.

(III) What we mostly have to resort to, in the class of cases now under consideration, are indirect means. We are

still, be it remembered, speaking of Observation as distinguished from Inference; so that the question before us is this: Since we cannot enlarge at pleasure any assigned object, so as to enable us readily to recognize its properties by our senses, what is the best available substitute for such a power? Apparently the next best thing to controlling the objective facts is to deal with those on the subjective side: in other words to improve, or aid, our own powers of sense. This opens out a wide range, extending from such simple implements as have long been familiar to everybody, up to the most refined inventions of mechanical genius. In fact we need not, for our present purposes, stop short at such a barrier as we find at the limits of the possible; but may, as we shall presently see, profitably extend our speculations by considering the introduction of still further improvements of the same kind in the region of the intellectually conceivable.

As specimens of the more familiar of such contrivances we may of course take any applications of lenses, such as the microscope and telescope. As we are not concerned here with any mechanical or technical details, it is enough to remind the reader that all which is attained by these instruments is the practical intensification of the sense of sight as regards magnitude. In them therefore we have the best available substitute for the direct enlargement of the magnitude of the object. They represent of course a very imperfect substitute. All that a microscope can do is to enlarge the visible aspect of the body in *two* dimensions; for, inasmuch as only one layer can be in focus at once, our perception of *depth* is very imperfect, compared with that which we can attain of ordinary visible objects through the aid of the adjustability of the focal length of each eye, and of the mutual inclination of the optic axes. In fact all accurate observation of objects by high power microscopes is observation of sections only, in other words of mere surfaces. So if we want to know the solid constitution of any very small body we must either build it up by a succession of such sections, or cut sections in various directions.

Another necessary defect, from our present point of view, in microscopic inspection of a body, is that although our sight is the principal organ by which we appreciate magnitude of three dimensions, it is by no means our only organ. For small objects

touch, and for large objects, locomotion, are essential components of the total impression of shape and magnitude; and where we cannot appeal to these directly, as in the case of inaccessible objects, we shall find that an imaginary appeal generally plays an important part in the aggregate impression.

Although therefore we may roughly describe the microscope as rendering the same service as would be rendered by an enlargement of the object, or diminution of the observer, we must admit that it really falls far short of this. If we want, say, to picture accurately to ourselves the shape and constitution of the growing point of a root, we have to cut a variety of sections of it in various directions, and by the aid of these, and by observation of the outside, we build up our aggregate conception. But any minute insect gifted with organs analogous to ours, would in a few seconds obtain a far more accurate picture of the relative shape and size of the object than we can thus secure:—how much we might learn from the animalculæ if only we could strike out some mode of communication with them! Even the one sided communication which should consist in our acquiring their perception at second hand from them would be invaluable to us.

In respect of none of the other senses have we made any serious advances in the direction in question. In respect of touch, the sense which stands second in intellectual importance, it would really seem from the nature of the case as if no advance were possible. We are here in direct contact with the object, instead of cognizing it through a medium; and we cannot well conceive any instrumental arrangement which should put us into an analogous position to that of any small insect, as regards the power to handle, and move about, a minute object; as we do secure an analogous position as regards the power of looking at it.

The next important sense is that of hearing. Every one knows the nature of one instrument used to aid us here, viz. the ear-trumpet. This does not however offer so close an analogy to the microscope as might be hastily supposed. That is, it does not in any way 'magnify' an object. What it does is to *intensify* it. All that is effected by the ear-trumpet is what we are so accustomed to in the case of sight that we there take its feasibility as a matter of course. If we cannot read in

a corner of the room we move at once to the window: when it grows dusk we merely wait for lights. It is the easiest thing possible to increase the light and thus to intensify the visual impression. But in the case of sound we have no such facile resource. We cannot, that is, increase the pressure or density of the air at will so as to make the sound waves more intense. Accordingly we have to resort to the ear-trumpet, which gathers up the waves of sound over a considerable volume and conducts them to the auditory nerves. The real analogy therefore here is that of a lens, not put to the eye for the purpose of magnifying, but held (as we sometimes find it convenient to do) between the candle and the object, for the purpose of increasing the light which falls upon the latter.

Hooke, that most ingenious inventor, the contemporary of Newton, has thrown out various suggestions, some of which it seems rather strange should never have been resorted to:—"there may be a possibility that by Otocousticons many Sounds very far distant...may be made sensible; at least the Noises of Thunder might be discovered at a much greater Distance than it can be by the Ear without these Helps, and thereby perhaps the Variations and Changes of the Weather might be predicted much longer beforehand than now they are, and Ships at Sea might perhaps discover an Enemy of Weather coming by the Hearing as well as they can now discern an enemy's Ship by the Light".[1] The barometer has partially superseded any necessity of this kind as regards the weather; but it does not seem at all clear that some kind of reflecting mirror,—such as we are all familiar with in the case of a whispering gallery or similar contrivance,—might not be constructed, which should be really serviceable in giving notice of the approach of a war ship to a hostile port. A multitude of other suggestions are offered by Hooke in the same work, one of which seems a faint anticipation of the stethoscope as an aid to inferring the state

[1] *Method of Improving Natural Philosophy* (p. 39, ed. 1705). It is well known how rapidly and clearly sound is conveyed through water; and by the employment of a suitable receiver for gathering up the sound waves we could thus receive intimations of an approaching object,—say a steam-ship,—at great distances. (In fact this suggestion has been more than once made in recent times; and I believe that extensive experiments are now being carried on for the perfecting of such methods.)

of the heart or lungs from our perception of the sounds produced.

Hooke has also raised some similar questions as regards our sense of smell, in which no one, so far as I know, has thought it worth while to follow out his suggestions into practice. If however it were found possible to quicken, on occasion, our sense of smell by some instrument analogous to the ear-trumpet, such a resource would surely be most useful. We know, for instance, that sewer gas is dangerous to health. But if our sense of smell for it could by any device be so quickened that the perception of its presence should be as far ahead of its power to hurt as is the case, for instance, with coal gas, our houses would be much safer to live in than they now are.

It was remarked above that what the ear-trumpet does is not to magnify but to intensify. This raises a somewhat novel enquiry, namely, what is the nature and extent of our power of enlargement of events in *time* as compared with our power over objects in space?

In the first place: can we actually 'magnify' an event, in other words make it take a longer time in happening? Certainly we can, and to a much greater extent than was possible in the case of space. What is wanted is of course proportional diminution, throughout all its portions, of the speed with which the event takes place: without this there would be what may be called time distortion. And this we are frequently able to secure, at any rate to some extent. For instance in the case of processes of our own, or those over which we have much control, we can often go through them more slowly: possibly as slowly as we wish. Thus a handicraftsman, or a conjuror, who wants to explain to us a process which he commonly performs too quickly for our eyes to follow, will go through the same process more slowly. And the speaker of a foreign language who wishes us to follow him, may have to utter his words and sentences much slower than he habitually does when speaking with a fellow-countryman[1]. But when we come to deal with

[1] Indirectly the phonograph can effect something for us in this way already, and it is quite possible that it may do much more. We may, that is make it repeat the spoken sentence more slowly if we please. At first sight it might seem as if we could thus 'magnify' to any extent, but apparently the quality of the sounds would be changed by their slower production.

other things than our own voluntary actions we lose this power. We cannot make a horse trot or canter slowly, and therefore, till instantaneous photography was introduced, it was a disputed point how the legs were moved relatively to each other. So with projectiles. If these could be made to move quite slowly men would not have remained long in doubt as to the nature of the path described by them; nor would artillerists be somewhat in doubt, as they still are, as to the exact direction of the axis of an elongated rifled shot during the course of its flight.

There exists therefore a very large field of events in which the rate of change is so great, or the whole time occupied so short, that our powers of observation are altogether baffled. In these cases therefore we stand much in need of some instrument, corresponding to a microscope, which shall be so contrived as to diminish the rate at which the successive brief and rapid changes in the stages of the event reach our organs of sense. Transferring the language employed in one order of considerations into the domain of the other, we may say that the power of magnifying a few hundred or thousand diameters, in respect of time, would often be of great service to us. It would make much difference in the completeness and accuracy of many of our observations, and consequent inferences, about very brief or very rapidly changing events.

Take, for comparison, some very minute object, say a pollen grain, and some very brief event, say a flash of lightning. What we do with the former in order the better to observe it is to enlarge the angular magnitude which it subtends upon the retina, so that the eye may be enabled to distinguish the space relations of its various parts. In simple words we make the object 'look bigger'. What therefore we want to do with the latter is to enlarge the time intervals occupied by the successive portions of the event as they reach the eye. In equally simple words with those above, though they do not happen to be in common use in this signification, we want to make the event 'look slower'.

The former of these wants is happily satisfied by the invention of a system of transparent lenses which spread out the rays of light in space. Can no sort of glasses be contrived which shall spread them out in time, if one may use such a

phrase? Unfortunately not, so far as we know; though there is nothing here which need deter the scientific imagination from trying to conceive the existence of such a contrivance. Indeed we should not thus have to do more than postulate the existence, in an extremely high degree, of properties which are undoubtedly possessed in an extremely low degree by various substances at present known to us. It is a fact that light travels less rapidly through dense glass than through that which is rarer. Take then a sheet of glass of which the density increases uniformly from one end towards the other, and look at the flash as it passes across the glass. If only the consequent delay thus brought about could be made sufficiently great, the desired result would be secured; for the whole duration of the flash, and that of each portion of the duration, would be proportionately lengthened out. If we could thus secure a delay of one second, and if the whole duration of the flash were the 10.000th of a second, we should have obtained a magnifying power of about 10,000 diameters, so to say. If the brief event were comparatively stationary in space, like a short electric spark, then we should have to move our glass very rapidly before it, instead of leaving it to move before the glass. We might then 'observe' the flash or spark as we now observe the minute object through our microscope.

These last remarks may seem to belong to the region of dreamland, but they do not the less demand the attention of those who have to speculate on the nature of Observation and Inference and on the means of improving them. Even in the most mechanical employments, and under the strictest confinement to practical aims, a man will never fully understand how his machine is working within its ordinary range and conditions, unless he also knows what it would do under conditions which he will probably never see realised. Our knowledge can never be sound and accurate about what does happen under present conditions, unless we make it embrace also a good deal about what merely might happen under conditions which do not exist. When therefore we are professedly dealing with speculation rather than with practice, the necessity of thus freely extending the limits of our illustrative hypotheses becomes urgent. Our observation and inference are carried on under certain conditions of space and time. Some of these conditions seem absolutely

inseparable from the very nature of our faculties, and cannot rationally be tampered with, even hypothetically. But there are others amongst these conditions,—here referred to, for want of a better term, as involving our control over space and time, —some of which are within our power to a certain small extent to modify and amend. With the nature and consequences of these the Inductive logician is bound to render himself familiar, both as they are now and as they may be in the future; but he will hardly succeed in doing this unless he is also familiar with them far beyond this point, that is, as they are conceivable to the Understanding but as they will never be realized in practice.

CHAPTER XXIV.

THE IDEAL OF LOGIC AND METHODOLOGY.

HAVING discussed, so far, the methods available for advancing and improving our knowledge of nature, it remains now to say something upon the question which must clearly underlie every discussion about advancement and improvement: Whither are we going, and what point can we hope to reach? In other words, what is the ideal of Logic and Methodology? In saying this it must of course be remembered that an ideal is not merely something which we never expect to reach,—so much as this is commonly recognized,—but it is something which we cannot even expect clearly to assign or comprehend. It is not given to us by Revelation, but is the outcome of our present powers of thought, and is therefore conditioned and limited by those powers, and will itself be modified by their exercise. The horizon towards which we walk is never the same for long together. Accordingly, the utmost which we can expect to do here is to indicate what, with our present lights, is the best to be hoped for and aimed at.

There seem to be three main heads under which this question may be discussed; according as we attempt to ascertain the extent, the accuracy, and the certainty, of that kind of knowledge at which Inductive Logic aims.

(I) As regards the extent of our knowledge. Here again we may subdivide, according as we look at this rather from the concrete or the abstract point of view.

(1) What we want to do, I apprehend, in the concrete way, is to be able to call up before the mind, on demand, any fact or group of facts, though these may be separated from us by any distance of time or place. This desideratum seems to belong

to the province of Induction, as commonly understood, and amounts to a wish that our inductions may extend without limit of distance or direction.

Perhaps we should explain ourselves better here if for 'Induction' we were to substitute that 'Indirect Observation' which is often recognized to be the signification of Induction. When we thus speak of calling up concrete facts and groups of facts, what we mean is simply this. Taking the ordinary interpretation of Observation and Description, we know what it is to observe and describe any actual occurrence. We form a sort of picture, in all its various details, of a number of simultaneous and successive facts, and we then give a verbal account of this picture in order to raise a corresponding notion in the minds of those who hear us. We well know that any so called observation itself involves a very considerable amount of inference or assumption; but, taking it as commonly understood, what we should aim to do is to be able to call up a picture of the same kind, of which the events referred to, occur or will occur, at any time or place which may be indicated.

As regards our present attainments in this direction, we are familiar enough with this standard in narrative history; for we there have, in relation to many important times and events, a continuous description which so far as it is accurate presents to us what would be seen by any one who was living through the times in question. So again with the physical characteristics of distant countries at the present time. The picturesque and accurate traveller will give us a lift into another climate, and enable us to see through his eyes what we have not the opportunity of seeing through our own.

Well, what we want to do in this way is to possess the power of calling up any such scene by our reasoning processes, without the necessary intervention of a witness, dead or living. Perhaps the only case in which we have obtained at present this power of concrete representation is in that department in which the concrete, by its tenuity, is really little more than abstract. We refer here to Descriptive Astronomy, for all which this science takes account of is the relative positions, motions, brightness, &c., of the various heavenly bodies. When we look out on a dark night we can readily take in most of these facts in what may be called a concrete picture. The astronomer has so

thoroughly mastered the laws of motion that if we were to ask him to give us a similar picture, drawn for any given time,—say at such a day and hour 25,000 years ago,—he could do it, and he could do it indeed with much more accuracy than any one of us unskilled persons would be likely to attain by use of our own eyes.

This is clearly the sort of knowledge, also, at which the geologist aims. He too would like to be able to construct a map for any assigned time and place in the remote past,—or, for that matter, in the remote future either. If we called for such a map, of Europe say, as it was a hundred thousand years ago, with indication of the coast lines, rivers, lakes, forests and so forth, he would probably attempt his best to do this, and he would feel that he was not working quite in the dark. He would certainly admit that such a claim was well covered by the ideal of his subject as entertained by him.

At present we are not discussing how all this is to be done, but are merely indicating the result as a desideratum. We want to be able to call for any portion of physical history as a man who had a living Macaulay at his elbow might have called for any portion of the public history of England, say, in the time of the later Stuarts. We indicate the time and place, and the required limits, and we want at once to be furnished with an account, on any scale we please, of what then and there went on. I may want to know, for instance, what is the condition of things under a volcano: that is, I require a sort of physical picture of the inside of the mountain and of the ground in its neighbourhood. This wish we ought to be able to satisfy, by describing that invisible state of things in much the same sort of way as a reporter would describe the physical appearance of the outside: the clouds, flame, flow of lava, and so forth. Or again, I want to know the general appearance of the Newcastle coal-fields and their neighbourhood at the time the forests were growing there. We can already do something in this way, but we want to do much more. We want the appearance of the country, logically or inferentially 'reported on' in respect of a multitude of details, just as we could get a report of the Tierra del Fuego or the Galapagos Islands at the present time.

(2) But such a reproduction of the remote, in a concrete form, is by no means all that we want. In the first place we

know of no means, generally speaking, for attaining it directly in that form. Our only available resource for securing a group of details, in many cases, is to get at them separately,—that is, more or less as abstract qualities,—and then to put these together. As was pointed out, when we were dealing with the so-called Four Methods of Inductive Inquiry, we can seldom get at our facts in this direct concrete way except when they are such simple events as happen repeatedly, so as to furnish us with a number of parallel cases. When the facts are rare or complicated we generally have to go to work by a much more analytical method. For instance, if we insisted on having set before us the state of things at the centre of the earth, we could not possibly obtain it directly in a concrete form, as we could obtain an account of what things are like at the North Pole from the first successful party of explorers. We should have to piece together several different qualities or conditions, such as temperature, pressure, and so forth, and then construct the concrete phenomenon by aid of these various elements.

But even if this were otherwise, anything approaching an ideal knowledge of nature would demand a great deal more than a mere narrative of facts as they occurred or were observed. Suppose that we really could summon up at will, in all their multitudinous complexity and variety, the details of some remote occurrence in the physical world, as in the magic picture of an enchanter's mirror. We might find ourselves placed in the position sometimes assigned in story to those who appealed to supernatural aid. We might be taken literally at our word, get what we asked for, and yet be no better off than we were before. It is quite possible to be smothered in the midst of an excess of details, owing to our being unable to marshal these details in order and to put an interpretation upon them. We certainly could not hope to be better off when thus obtaining a picture of the whole at second hand than we are when observing the facts at first hand for ourselves. Now when we have the facts face to face, we mostly have them in the form which I understand to be denoted by the true sense of the familiar expression "an open secret"—that is not a puzzle with a simple and obvious solution to it, but rather one in which all the facts are there patent before us, but to which we cannot apply the interpretation. The box stands open, sure

enough, but the contents are too infinitely complex for us to be able to say what is in it.

What is wanted therefore is a complete system or apparatus for breaking up this complex whole into parts, and for arranging these parts in such a way as to enable us to control and manage our materials. That is, both Analysis and Synthesis are required. The analysis here takes mainly the form of resolution into what are called Laws of Nature. The synthesis takes mainly that of Classification.

As regards the Laws of Nature, and the logical ideal to be set up for their treatment, it is not unlikely that the reply which will be given off-hand is, that we must look forward to their reduction to the smallest number, and wherever possible to one single law. Such a statement however requires to be looked at somewhat more closely, for it lays itself open to a possible charge of confusion between what is actually simple on the part of nature, and what is found convenient on our part when studying nature. We have seen on a former occasion (Chap. XVII. p. 427) that the maxim that 'Nature always works by the simplest means' is better interpreted as implying no more than that the simplest supposition is the best for us to commence with; and it is possible that a similar transfer of signification may be found convenient in the present case.

Let us take an example for consideration in which it is generally agreed that we have at present effected this simplification to the utmost possible point, namely that of the Laws of Motion as expounded by Newton. When we are considering the behaviour of bodies which do not come into contact with each other, we find that their movements may be accounted for by the assumption of the three well known Laws of Motion together with the Law of Gravitation. That is, if we are considering the path of, say, a planet when revolving round the sun, we find that, given the mass and the initial distance, direction, and velocity of projection, we can by aid of those four laws calculate the entire path and therefore determine every subsequent position. So far nothing more is involved than a case of "Explanation", of the sort discussed in a previous chapter. And no attempt worth considering here has been made to simplify the three first of these laws, or to reduce their number. But as regards Gravitation it is otherwise.

Attempts have been constantly made to reduce this, if possible, under one of the others or under some combination of them. Consider but one of these attempts, that which seeks to explain the mutual gravitation of any two bodies by the assumption that space is filled by innumerable solid particles in incessant motion, and that the two bodies by mutually shielding each other, tend, so to say, to be pounded towards each other.

That such an explanation, if it be true, lies within the ideal of science is of course indisputable; and if it should ever be established we shall have made a further step in simplification. But this is not quite the question now before us. The point here is whether the fact that such a result would effect a simplification gives any à priori evidence in favour of the theory: If true, the theory would serve to simplify matters, but does this make it more likely to be true? The opinion which on the whole till lately probably occupied the field in regard to Gravitation is that which regards it as an ultimate fact that all bodies, when left free, tend to approach to each other with assignable acceleration. Against such a supposition, however, many minds violently rebel; but except from a natural wish to simplify, it is not easy to see on what exact ground. The mere objection that such an ultimate law is 'inexplicable' is surely invalid: precisely the same may be said against the three laws of motion; and a slight diminution in the number of our inexplicable assumptions is no very weighty motive.

It may be urged that the grounds for accepting the Darwinian Hypothesis were of much the same character as that discussed above. The doctrine of separate creations for the fauna and flora of each separate Ocean Island was long accepted by most naturalists as an ultimate fact, but this assumption gave way at once before the suggestion of an explanation which resolved the many distinct suppositions into various cases of one group of causal influences. The analogy here however does not seem a very fair one, because the simplification was of such a colossal kind. It did not merely concern the reduction of a small number of agencies to one somewhat smaller, but it made a clean sweep of a simply *indefinite* number; for the natural demands of those who claimed separate creations for each species, and areas of creation for each locality, had a tendency to

grow without limit as our knowledge of the world present and past was extended. And in addition to this the invocation of continual creations was the introduction, not of one more assumption, but one of a kind which was *sui generis*. A fairer analogy to the doctrine formerly prevalent about the Origin of Species would be found in supposing that men had once taken it for granted that every shooting star was just created at the moment it first came into view. Such a theory naturally would not find much support, as against one which suggested that known laws of motion and matter would account for the sudden flaring up of meteors when they came into contact with the atmosphere of the earth.

As regards then the presumption in favour of simplification it seems to me that we can say so much as this. Given a certain set of facts it is our obvious duty to simplify the arrangement of them as much as possible,—as we shall see again presently when we come to deal with classification,—but this obligation concerns our way of viewing the facts rather than their actual nature. Where we are able however to effect a very great simplification by some alternative supposition, and especially when we thus get rid of what we call independent acts of creation, there is a strong presumption in favour of so doing. But when the difference amounts to nothing more than the ability to get along with an ultimate law or two the less, the presumption in favour of the *truth* (as distinguished from the convenience) of such a supposition, seems to me to be comparatively small.

Two examples,—one which has long been recognized as scientific, and the other of a class which has only just begun to claim such recognition,—may serve to explain what is here meant. The first is that of Gravitation; briefly referred to above. One view is that we must admit as an ultimate fact that all bodies tend to move towards each other; that we need not assume that there is anything either between them or behind them in order to make them do so. When to this view is opposed the counter supposition that the movement is caused by the pelting of atoms from behind, or the communication of pressure or vibration through an intervening fluid, every one would recognize that, given the existence of such media and assuming that the facts would be equally well accounted for in

such a way, it is desirable to adopt such a simplification. It is our scientific duty to do so. But I cannot see that the fact that we should thus furnish nature with one set of ultimate laws the less is itself a serious argument in favour of either theory[1]. The other example is that of the so-called Telepathy, as described and supported in the *Phantasms of the Living,* and other more recent works: the doctrine namely that one mind can operate in certain cases directly upon another without the intervention of any physical medium. (I am not arguing in favour of this hypothesis, which seems to me not yet to have become, strictly speaking, probable.) Here, as before, it does not seem to me that the mere difference between one more or less of ultimate postulates or laws counts for much. We must suppose, of course, that the facts have, as regards their broad outlines, been established beyond doubt, and beyond reasonable explanation from chance coincidence; and the alternative will then be between assuming that it is an 'ultimate law' that one mind will tend to induce a similar state (under certain very special circumstances) in certain other minds, and assuming that the observed coincidence or sequence is a result of various other 'laws', such as fraud, hallucination, and so forth. The consequent detriment of a mere addition of another ultimate law to those (say) of mental association, or of physical and physiological sequence, does not seem to me to count for much.

Turn now to Classification. After the long chapter already devoted to this subject we need not say much about its ideal character; partly indeed because, as will be inferred from what was there said, we are so little competent at present to forecast its future. We saw that the principal improvement already effected consisted in the substitution of a 'Natural' for an 'Artificial' classification; but we also saw that when we endeavoured to bring to light the real distinction between these systems several accounts might be given. It might be said that what was aimed at was the arrangement of the species in accordance with their actual genealogical affinity. If this

[1] Is there not indeed something essentially arbitrary, and, to borrow the theological term, anthropomorphic, in this whole way of looking at things? Are we sure that the solution, which is easiest to conceive by our faculties, or most economical of statement in our language, is necessarily the simplest in nature?

account were adopted, the ideal of classification would become a 'History of Creation',—such as, in fact, Hæckel has already attempted to sketch out. If however we preferred the account adopted by Mill, that the object of classification is to arrange the objects in such a way that each class shall comprise the greatest number of common attributes, regard being had to the importance of these attributes; it seems by no means clear what would be the final outcome of such a process. Importance is a very fugitive thing, depending upon the habits, studies, and tastes of men, and on these points no estimate, which the present is competent to frame for the future, is worth much.

It seems therefore preferable to regard Classification as a means to an end, that is to the effective study and control of the objects in question, and to anticipate that the final outcome of it will not so much take the form of some one arrangement universally accepted, as of a continually increasing number of arrangements devised to meet the ever extending wants of students.

An illustration of what is here meant may be gathered from a comparatively trivial subject, purposely chosen for that reason, because the very shallowness and artificiality of the subject-matter enables us to look with some confidence further ahead than if we were dealing with the profound problems underlying the biological and social studies. Suppose, for instance, we have a large number of names of inhabitants of some county, or students of some school or college. What we want, of course, is complete and ready control of this mass of material; that is, given any characteristic whatever, we wish to determine at the shortest notice which of the objects possess this and which do not. For this purpose we clearly want a considerable number of arrangements. We should begin with an alphabetical one, because this is the readiest means of picking out a person by his name. We should want a chronological order, for we might be asked who were the contemporaries of such and such a man; and here the alphabet would not help at all. We should want a topographical order, so that those interested in a given place might know what men were connected with that particular place. We should want the men arranged in accordance with their professions, so that the historian of the church or the bar could pick out his repre-

sentatives without the labour of working through the entire list. We should want them grouped by their dates of death and places of burial; by the localities where their wills were proved; by such facts as where and to whom they were married, and so on. In a word, nothing would represent finality here except the attainment of a distinct arrangement in accordance with every distinct characteristic which we had occasion to consider. Our only way of securing completeness would be, not to make a compromise amongst these various attributes so as to secure the best possible single arrangement, but rather to aim at a complete assortment of separate arrangements each involving only one characteristic.

There seems to be some ground for thinking that this will be the tendency in more serious branches of study. Take the case of Botany. It is surely unlikely that the systematists will go on for ever endeavouring to put the finishing touches to the so-called Natural System. Those who want to study genealogical affinity will prefer to do so directly, without the constraint involved in the traditions of classification. And the innumerable demands of other students will have to be met by separate arrangements, each devised for their own private satisfaction. The topographical botanist will have his arrangement,—such as H. C. Watson attempted in his *Cybele Britannica*,—in which local distribution will be the determining attribute. The pharmacologist will have his arrangement; the commercial man his, and so on. There seems no practical limit to the number of such classifications,—whether they go by that name, or are termed Indices or Dictionaries, or what not,—which would be really serviceable in the great objects of all classification, namely that of saving time in looking for something we want and keeping clear control over a multitude of details. The ideal therefore would be sought in a continual addition to these labour-saving resources; that is, in the accumulation of special classifications rather than in the attempt to agree in the construction of some one dominant and exclusive arrangement.

II. So far we have dealt mainly with qualitative considerations. When we come to those which are quantitative we feel on somewhat clearer ground as regards our general aim and the extent to which we may hope gradually to secure it. What

we want, in a word, is to "take out the quantities" in respect of all the phenomena of nature. This seems to lead to two subdivisions.

(1) In the first place we want a complete set of units or standards. Those in present use are not only occasionally ill-chosen in themselves, but there is not as yet universal agreement in their acceptance. The resources of scientific inference are clearly imperfect until these defects are remedied. Then again there is still an immense deal to do in the way of connecting the various units with each other, so as to express one in terms of another. Two familiar examples will serve to show what is meant by this,—Kater's determination of the length of the seconds pendulum at a given latitude, and Joule's determination of the mechanical equivalent of heat. Each of these results was intended to decide the relation, with the utmost attainable accuracy, between units of distinct classes of phenomena. The same will have to be done in every other direction, as in fact it is being done (to give but an instance) in respect of certain electrical and magnetic constants.

But this is only a portion of the work before us. Our task, as regards the elements or raw materials of scientific calculation, is not completed so long as any measurable magnitude whatever is left without its suitable unit, direct or indirect. The sort of problems which this opens out before us was indicated in the chapter upon Psychical Units. The same thing will have to be attempted, if the materials of our knowledge are to be rendered complete, in respect of every markedly distinct kind of sensation, feeling, and emotion.

(2) And having obtained our suitable measures for every separate class of things, we want, of course, to apply them to every possible object in each class, or where these are indefinitely numerous, to the average of each class. All this is too obvious to need discussion. Thus in the case of the earth we want to know its exact size and shape, its mass or specific gravity, the time it takes to revolve, the distance between different points on its surface, the depth of the sea, the height of the mountains, and so on. In a word we want a series of exact measurements of all its physical characteristics. Similarly in the case of groups, say of men, we want a set of measurements, of like kind, of the *averages*: we want to know

the mean length of life now and in former times, here and in other countries; the mean stature of different races, professions, classes of men, and so forth. We shall not have obtained anything resembling an ideal in this department until we are furnished with a complete set of measurements of every measurable element, either individually or through averages, which we find in nature.

III. One more requirement still remains to be indicated. Our knowledge is not like an outline sketch, of which the exact direction and magnitude is all that we require to determine; we must also insist upon having the *shading* put in. By this is meant that we must know how certain our conclusion is, and what are the limits of probable error. This condition requires somewhat closer examination.

It has been already mentioned several times, and especially in the Introductory Chapter, that the final aim of all science is complete certainty, for or against every assertion. The outcome must be, so to say, a mere black and white sketch, in which everything is either accepted or rejected. But here, as well as elsewhere, there is an ideal to be kept before us at every step, as well as one to be pursued in the remote future. And in the case in question this must take the form of urging us to endeavour to adjust correctly the degree of certainty which, according to our present knowledge, ought to be assigned to every conclusion.

Our present attitude towards very many of the nameable things which we have occasion to refer to is one which could never be accounted for except on historical grounds. If we turn to the dictionary, as to the receptacle from which our stock-in-trade of words is to be drawn, we find it filled with many words for things which no living man now believes in, with many which we are divided about, and with many which we all without exception admit as actually existent. But we have no means of distinguishing between our mental attitudes towards them, unless we mark off those which are discredited, by expressly labelling them, or in some other way describing them as imaginary. And no one feels any incongruity in making them all alike subjects and predicates of propositions: he leaves it to the context to decide whether or not his belief in them is implied. In illustrative remarks, for instance, no

such belief is implied, and unless we put the two entities side by side into one sentence, no incongruity is felt. (I presume it would verge on the humorous to speak of anything 'rising from its ashes, like the Phœnix or the town of San Francisco'.) Such an attitude however seems to me unintelligible except on historic grounds and as a temporary phase. We continue to speak of the impossible, the doubtful, and the actually existent, in the same grammatical terms, because many things which at the present day are supposed to occupy one of the two former classes were understood to stand in the third when the terms were first introduced.

It will hardly be maintained, by anyone, that this is a thoroughly logical or scientific attitude. If language and thought were to be reconstructed, we should presumably expect that every one would find to hand some appropriate inflection or modification which would show at once to which category the term in question, in his opinion, belonged. When he spoke of ghosts, of the soul, of heaven or of hell, we should know from his form of expression whether these were regarded by him as realities or not.

To work out any such suggestion as this would of course be merely fanciful. All that we can here indicate as a rational ideal is final agreement amongst mankind on all these (at present) doubtful points. And if there is any validity in Logic, and any substratum of reality about our thought, this final agreement must be admitted and hoped for as our ultimate goal.

Short of this goal, what we desiderate along our path towards it is a due estimate of probabilities. As this subject is generally treated apart from ordinary Logic, and as I have myself written tolerably fully upon it elsewhere, a very brief indication must suffice.

There are then a very great number of facts about which that final agreement, for which we hope, does not avail us anything, because we want to evaluate our decision at the present moment. A man in good health would doubtless like to know whether he will be alive this time next year. This fact will be settled one way or the other, in due time, if he can afford to wait, but if he wants a present decision Statistics and the Theory of Probability can alone give him any information. He learns that the odds are, say, five to one that he will survive;

and this is an answer to his question as far as any answer can be given. Statisticians are gradually accumulating a vast mass of data of this general character. What they may be said to aim at is to place us in the position of being able to say, in any given time or place, what are the odds for and against any at present indeterminable fact which belongs to a class admitting of statistical treatment.

Again; outside the regions of statistics proper,—which deal broadly speaking with events which can be numbered or measured and which occur with some frequency,—there is still a large field as to which some better approach to a reasoned intensity of belief can be acquired. What will be the issue of a coming war? Which party will win in the next election? Will a patient in the crisis of a given disease recover or not? That statistics are lying here in the background and are thus indirectly efficient in producing and graduating our belief, I fully hold; but there is such a large intermediate process of estimation, and such scope for the exercise of a practised judgment, that no direct appeal to statistics in the common sense of the term can help us. In sketching out therefore the claims of an Ideal condition of knowledge, we ought clearly to include a due apportionment of belief to every event of such a class as this. It is an obvious defect that one man should regard as almost certain what another regards as almost impossible. Short therefore of certain prevision of the future we want complete agreement as to the degree of probability of every future event:—and, for that matter, of every past event as well. Accurate quantitative apportionment of our belief,—so far as this is intrinsically possible in any case,—can only be hoped for where complete statistics are available; but we ought to look for general agreement as to the more and the less probable. To take an illustration. No one without a thermometer could undertake to say how much higher was the mean winter temperature of London than that of Cambridge; but we should all agree that that of Edinburgh was lower than that of Penzance. The present state of things on most questions of politics, philosophy, and religion, is much as if the mean climates of London and Cairo were still in dispute, and were decided by personal sympathy and party considerations.

Once more. Even in questions of pure measurement of

some quantity which it would be universally agreed admits of accurate determination, in the sense that it really has a determinate value though we have not yet ascertained the amount, there admits of much advance in respect of the probability of our assignment. Take an example from the practice of astronomers. If they are asked to assign, say, the angular distance of one star from another, they would begin by doing this to a degree of precision which in itself far surpasses anything which the layman can realize. But in addition to this they would supply a correction assigning what is called the 'probable error' of the previous assignment. (They would, for instance, give the distance as $15^\circ\ 14'\ 22''{\cdot}7 \pm 1''{\cdot}7$, or whatever it might be.) This indication of a probable error is only another way of describing the degree of certainty of the measurement obtained, but it is the sole way of doing so. As we are dealing with a quantitative, and not a merely qualitative element, it would not be possible to give a measurement and then say that the chances are 5 : 1 that this is accurate; it is quite certainly *not* accurate if we are sufficiently punctilious in our determination. All that we can do is to assign a measure of departure, and then say what are the odds that we shall be within these limits; or conversely assign certain odds, and then say what is the margin of error which will secure those odds. The latter is the course adopted. The odds chosen are *even*, and the error is assigned within which it is thus an even chance that we should attain.

Formally speaking, Astronomy does really seem to have attained its ideal here, and the same standard is reached in many kinds of geodetic measurement. A magnitude is assigned according to the utmost accuracy attainable, and then (by aid of the very attempts at obtaining this) another is assigned which indicates how certain we ought to feel as to the accuracy of the former. And nothing less than this ought to satisfy us, I apprehend, as our ideal wherever we are dealing with quantitative results. We ought to endeavour to determine not only to the best of our power the actual magnitude, but also the value of that determination itself; that is how likely it is that we shall be within an assignable distance from the truth.

CHAPTER XXV.

SPECULATION AND ACTION; OR THE LOGICAL AND SCIENTIFIC VIEW OF THE WORLD, AS MODIFIED BY OUR PRACTICAL TENDENCIES[1].

Up to this point we have adhered as consistently as possible to that objective scientific view of the Order of Nature, which was fully described in the introductory chapter as being the view which seemed on the whole the most appropriate for Inductive Logic. On this explanation we suppose the observer, who may be the logician, to be looking on passively at the evolution of events with which he does not in any way meddle. He may observe, judge, infer, and record, as may an outsider at a game of whist; but it would be as great a breach of etiquette, in the one case as in the other, for the observer to intrude himself into the position of an agent, or in any way whatever to attempt to influence the course of events, instead of remaining as a silent spectator.

This is the point of view of Science, pure and simple. Contrasted with this is the point of view of Art in the wider sense of the term; that is, in the philosophical as contrasted with the æsthetical sense. The general nature of this contrast will doubtless be understood by the reader without much explanation, so that very few words need be added by way of exposition. By Science, briefly speaking, may be understood a coherent and methodical body of generalizations about matters of fact; by Art, a similar body of rules or directions for securing some end or class of ends. The former deals with what *is*; with what exists in great part irrespective of our wishes. The latter deals rather with what ought to be, or with what is

[1] The substance of part of this chapter appeared in the first two editions of my *Logic of Chance*.

wished for; wished for, it may be, by the speculator himself or by others as represented and interpreted by him.

The opinion seems to be sometimes entertained that there must be an Art corresponding to each particular Science, and conversely. It is quite true that such a correspondence may sometimes exist; but to suppose that it should exist generally would seem to imply some error as to what constitutes the unifying principle in respect of each of these classes of generalizations or rules. What, for instance, confers its unity upon any Science? Broadly speaking, the fact that we are dealing with a number of objects so connected together by their recognized resemblances that we may regard them as constituting a class:—by 'resemblance' here being of course understood any connecting links either of origin, nature, or destination. When our accumulated knowledge about these facts becomes so large as to prove unwieldly, or too extensive for one person conveniently to grapple with, the class has to be broken up into smaller classes, connected by additional and more special resemblances. Thus Geology, or the Science of the past history of the surface of the earth, is gradually being differentiated into Petrology, Palæontology, Vulcanology, and so forth. Chemistry now furnishes two rather widely distinct branches, according as we deal with organic or inorganic matter. And a precisely similar process of gradual specialization is displayed in the evolution of Art, in the sense in which we have here understood it. Every reader of Herbert Spencer will be familiar with the nature and range of the process which that philosopher has so amply illustrated in his various writings.

Now it does not seem likely, in general, that the group of similar or connected attributes which thus unify the class of attributes for the purposes of science, should just happen to coincide with some one want, or set of wants, on the part of man; or therefore be suitable to form the province of one Art. It is hardly necessary, for instance, to point out that there is no such correspondence in the case of Geology. 'The past history of the surface of the earth' represents no special want in the mind of man, for the art of Husbandry deals only with a small portion of the topics thus combined. Consider again, the Art of Navigation. Here the aim or desire is that of taking a ship safely and speedily across the seas. Men wanted to do this long

before anything worth mention was known about the stars or winds or ocean currents. And therefore, corresponding to this wish, there is a real Art of Navigation, distinct from Astronomy and Meteorology and the other physical sciences to which appeal has to be made by the seaman.

There are no doubt certain peculiar cases, mainly inclusive of a few sciences of very wide import or abstract character. For instance, Political Economy, as it used to be commonly defined, deals with the actions of men in so far as these are devoted to the pursuit of wealth. This distinctly corresponds to an end or aim which is as simple and as permanent as almost any which can be mentioned. The reason why the subject is to be considered as a science rather than as an art is connected with some interesting considerations which will have to be noticed presently. But so much as this may be said here: that, in the wide sense above suggested,—that of National Economy, or a body of rules for enriching a whole nation,—the subject has seldom been cultivated at all. The actual rulers of most countries have had other ends in view besides that of rendering their own nation rich. On the other hand, in the narrower sense of domestic economy, or the rules for making the individual rich,—an art which is not likely to be neglected,—the generalizations and conclusions of learned authors are found to be of very slight assistance. But the ground of this distinction will recur presently for fuller discussion.

Another instance in point is that of Logic itself, as to which there is a very old dispute whether it is to be considered as an art or a science. It seems to me that Logic may equally well be regarded as either, but then it must be remembered that we are dealing here with a subject-matter which, regarded under either aspect, is decidedly abstract and artificial. 'The operations of the human intellect in the pursuit of truth',—to adopt a rather vague definition,—may certainly be made the nucleus about which the materials of a real science can be grouped. But, as may very possibly have occurred to the reader long before he has reached this chapter, the unity thus secured is somewhat artificial, and wide differences of opinion may exist as to the proper delimitation of such a science. When the same general object is regarded as an aim to be pursued, the same materials, or at least a large proportion of them, will

equally well group themselves together into an Art. Somewhat similar remarks may be made in relation to Ethics. Here again we are dealing with a subject-matter of enormous range, for almost every human action falls within its scope, and we find in consequence that the distinction between Science and Art is not very easy to trace. Such cases as these, however, must be exceptional. As a general rule, if we start with the conception of some end to be pursued, ultimate or subordinate, or some set of wishes to be satisfied, in ourselves or in others, we shall find that we have to make appeal to the cultivators of a number of widely distinct sciences, and to accept our results from them. And similarly, he who cultivates a science, if he begins to take into consideration the ends which it is likely to gratify on the part of mankind, will soon discover that these ends are very numerous and various. Each of the two main principles of unity leads to its own resultant aggregation, and is therefore best worked out in entire independence of the other.

There is an important consequence of the distinction previously indicated which deserves careful notice, as it is very generally overlooked. It is this. The conclusions of Science, are independent of the number of persons to whom these conclusions may be understood to be directed. This follows from its consistent adoption of the merely speculative attitude ; from the fact that its duty is to observe, not to advise. Ask the geologist to how many persons his conclusions are supposed to be addressed, and he would hardly know what we mean. He would probably reply that the truth of his generalizations is not in the least affected by such a fact as whether one person or a thousand are likely to accept them : that he addresses himself to the intelligent world at large, and the wider the circle he can succeed in influencing, the more completely has he gained his object. The purely speculative attitude of the scientific thinker, whether we consider the original author or the learners who study and accept his conclusions, is in fact an impersonal attitude: it exists entirely irrespective of the numbers of those who adopt it. The exponents of the science do not, in any direct way, influence the objective truth of the doctrines taught. Those who merely teach, teach in fact an altogether indeterminate number of persons.

But when, on the other hand, we come to advising or commanding,—that is, when we begin to trench on the department of Art,—such an enquiry as that above indicated becomes at once relevant and important. An art comprises a collection of rules, and in issuing a rule we must always be prepared to give an answer to the question, To how many persons do you intend this recommendation to be addressed? Often, no doubt, such an enquiry may be of no practical significance; but there are cases on the other hand in which the answer will seriously affect the soundness of the rule. That is, the rule would have to be quite differently formulated according to the number of agents to whom we suppose it to be addressed, or at least according to the number by whom we suppose that it is likely to be accepted and carried out.

Consider for instance the case of Political Economy. From the time of commencing to read the subject I remember to have been perplexed, as presumably many other students have been perplexed, in deciding how its doctrines or precepts were to be put in practice. No doubt some of its modern,—or, as perhaps we ought now to say, most of its recent,—exponents flatly deny that it is at all within its province to offer a word of advice; but surely this is rather pedantic and need not hinder us from attempting to extract advice from its teaching. It is unquestionably a Science of Conduct, that is, it comprises statements as to the actions which people in various positions perform in pursuance of certain dominant motives; and it enables us to predict many of the consequences of such actions. But suppose we wish to convert the Science into an Art: suppose we attempt to extract rules telling us how we *ought* to behave, out of the statements of the Science as to the consequences which follow from the way in which we and other men *do* behave? Of course for the transformation of a science into an art some end or motive must be supplied. But for this end we have not far to seek in such a case, for, as we have already seen, the end here is nothing else than the general principle which is supposed to confer its unity upon the Science,—viz. the pursuit of wealth,—so that the boundary lines of such an art and science will coincide, or nearly so.

To whom then is Political Economy supposed to speak in

the rules which we may draw for ourselves from its generalizations? Take such a case as the following. Suppose it to be admitted that what is called the Cooperative Store system, as distinguished from one carried on by retail traders, is an economic benefit to society, owing to the saving of the unnecessary cost entailed by an excess of middlemen. On the side of Science there is no difficulty. We can indicate the sort of difference which results when all persons adopt the new scheme, or when some only do so. But what is the *advice* corresponding to such conclusions? What rule is to be given? What sort of results follow, minute as they may be, if I personally frequent the store rather than the shopkeeper's counter? You cannot recommend each person to commence to do what would result in benefit if undertaken by all together, nor can you recommend all to adopt a practice simultaneously because advantage would follow if it were gradually undertaken by a few at a time. To say nothing of the disasters which might ensue to the retailers by the latter course, it is obvious that by setting to work in such a way the result might be that we should bring on a fresh train of events which would defeat the very object aimed at. The same difficulty would present itself whether our supposed student, who was so desirous to apply his science, took as his guiding motive the desire to increase his own wealth, or that of the nation at large: the generalizations laid down in his text-books would not help him, as an individual, in any direct way.

The above is merely offered as a single example. We shall have to examine similar considerations more fully presently in the case of Ethical rules, where we shall find that their whole significance, as means towards given ends, will sometimes turn upon the number of agents who may be assumed to follow them. It will be found in the case of many rules, I apprehend, that one condition of their successful operation is that they should be addressed to a certain assignable number of persons, or at least that some assignable number only should be expected to attend to them.

We must now proceed to discuss a step which from a speculative point of view is an exceedingly important one; that is, we must take account of some of the principal ways in which, intentionally or unintentionally, actually or hypo-

thetically, we are apt to step from Science into Art, or to confound their clear boundary lines. The strict province of the Inductive logician, as has been so often insisted upon already, is that of science pure and simple. But uniform and consistent adherence to this province is out of the question. Our science is meant to be put in practice. That is, the objects with which it deals will in the majority of cases either comprise human beings, who are therefore simultaneously agents as well as thinkers; or else it will refer to things and events in which human beings take an interest and with which they can to some extent interfere effectively. We cannot therefore propose to sit alone, on some lofty pinnacle, doing nothing else than inferring the past, judging the present, and forecasting the future.

I. The first class of such intrusions into practical life may be described as *actual*, that is, in opposition to such as are only hypothetical; and as *legitimate*, that is, as logically legitimate in the sense that they need not perceptibly disturb any forecast that had previously been made.

(1) Such intrusions may take place on widely different scales. A single individual, for instance, may study such and such a science, and then proceed to exercise his knowledge and exert his influence within his own little sphere of action. In so doing of course he trespasses from the scientific position, for he may actually alter the facts as they are presented to others. The world would not have been the same to any one else if he had not thus interfered with its career, and therefore the scientific conclusions for every one else are slightly altered.

The answer to such a suggestion as this is twofold. There is, in the first place, the ready reply of the practical man; that the results in question are of that minimal amount with which neither law nor science need trouble themselves. This is true enough in most cases, but a sounder reply may be given by an appeal to the theory of averages. What we are concerned with, in a very great number of cases, especially where our subject-matter comprises the various doings of men, is not so much the single action as groups or averages of many actions. No one has power, by his own individual conduct, to do any appreciable injury to an 'average' statement. Con-

sider the position of an official registrar, who is contemplating his statistical results of past years, and making his forecast for the year to come. He may be contemplating matrimony, or have made up his mind to commit suicide, before the next returns are published. According as he does or does not take the proposed step, the resultant numbers will be altered; and if these professed to be numerically precise it is clear that his own decision might falsify any inference which had been drawn from data which did not take it into account. But his conduct of course is a merely isolated event, whereas the statistics represent an average. Hence although his suicide, say, may be in a way formally unwarranted, owing to its being an intrusion into statistics which had not contemplated its occurrence, the event is at once merged in an average and disappears from notice[1].

(2) In the former case the change or innovation, if one may so call it, though real, was supposed to be insignificant in amount. Now consider the position of any one with sufficient power and influence in the world to make his mark felt. Suppose him to be a statesman, a conqueror, an ethical, social or religious innovator. The contrast between the purely logical or scientific point of view, and the practical, now becomes important; for such persons, if they act consciously, cannot adhere to the former standpoint. Whether it be the agent himself who is proposing to infer the future from a study of the past, or some observer at his side who sees what is proposed to be done, the new stream of causes and effects must necessarily be taken into account. Their calculation would take the following form. They might say, The forces at work, omitting the consequences of the agency now in question, would have resulted in such and such a development. Now the conduct of this agent is a modifying element, and one which we are bound to reckon with, but it is clearly one which the agent himself, when deliberating over it, cannot regard as a mere

[1] Remember that we are here speaking solely of the individual and of his actual or proposed action. In case it be replied,—Well but what if all other persons, or even any considerable number, were to adopt this line of explanation, and conclude that the results would still remain insignificant,—the answer is that such a hypothetical alteration introduces another set of considerations. It will be separately discussed a few pages further on.

inference from the past, as an outside observer might do. An Alexander or Napoleon, for instance, at the climax of his career, could hardly be conceived as inferring the future from the past by a mere process of logical calculation, without supplementing this by introduction of the personal factor which he contemplated supplying.

The above remarks are obvious enough, and are only introduced for the sake of what is to follow, as it seems to me that some of the consequences of the distinctions thus indicated have not been sufficiently attended to in speculations upon social matters. Take the case of Ethics. Conceive an individual imbued with the determination to prosecute to the utmost the greatest happiness of mankind. This is to be the guiding motive of all his conduct, but he is to follow it out on his own judgment and instigation; and we do not suppose this motive to be backed up by any such structure as, for instance, Paley introduced in order to enforce indirectly the pursuance of this common end by a direct appeal to the private ends of each individual. He need not be supposed even to have been taught Ethics, or indeed to know that any such thing as a System of Morality with a sanction behind it, divine or human, has ever been promulgated. His pursuance of the public good is to be simply a piece of policy,—unselfish policy,—directed towards this end.

Such an attitude is a perfectly possible one, and is indeed nothing more than the consistent adherence to a policy which most of us adopt from time to time and under special circumstances. It will serve to illustrate a point which was urged in the chapter on Hypothesis. The only opening for an 'if' on such a scheme would be in the ordinary contingency of such and such alternatives as actually anticipated. Our agent may reasonably say, 'If I do this, such a result will follow; if I do that, such another result,' and so on. But how could any opening arise for that other and wider scope of the hypothesis which is so commonly employed as a test, 'But if all men did this or that, what would then be the consequence?' Such a form of supposition need no more be resorted to here than it need be introduced by a man who was backing a horse at a race, who would hardly see the relevance of testing the consequences of his proposed step by enquiring what would follow

if all other men were to back the same horse. The conduct o our supposed agent, in respect of his moral career, would in fact be of the sort which the poet Parnell has described. A wanderer falls in with a mysterious stranger who accompanies him for some time, and whose actions grow more and more suspicious. He robs a hospitable guest of a valuable cup, and bestows it on a churl who had barely admitted them within his door. And so he proceeds until he finally murders a child who was his father's joy and pride. The wanderer's indignation is beginning to explode when the stranger suddenly assumes his true guise as an angel, and proceeds to explain how every step he had taken resulted in a clear balance of good to the parties concerned, and therefore, we may say, in the increase of the happiness of the world.

Now this, it seems to me, is exactly the sort of conduct our supposed schemer for the good of mankind, our ethical reformer who fought for his own hand, would be wise in adopting. No doubt the agent above described committed one very serious mistake, by performing his actions in the presence of an observer; a mistake only partially remedied by his subsequent explanation. But as we should not have heard the story unless he had done so, this customary dramatic device may be excused. A consistent scheme of policy directed to a certain end, but pursued by some being, however wise and good, who acts entirely for himself, on the one hand: and, on the other, a scheme directed towards precisely the same end but designed and promulgated by some central authority who proposes to guide and influence others, will lead, and 'ought' to lead, to widely different practices in pursuance of such an end. The latter may issue a set of rules such as, if followed by all men, would tend in the desired direction; and those who are subject to his authority have no business to look outside those rules. It is open to them to question the fact whether such pursuance by all would tend to the desired consequences, but when this is once admitted they have no call to calculate individual results. The former has no real need of rules at all, and the knowledge of what exactly other agents are proposing to do is of vital importance in his reckonings. The extremest illustration of the distinction between these two positions is given by contrasting what is reasonable on the part of a private soldier

in a large army, and on that of some guerilla chief who has a perfectly free hand. In the case of the former it is a step towards mutiny if he once begins to speculate whether his comrades will all obey some order which has been issued to all alike. In the case of the latter it is folly if he does *not* take into consideration precisely similar considerations. He and his fellow chiefs may have conceived the same plan, or received the same advice, say, to attack the enemy at some assigned time and place. But the knowledge that a certain number, and the rough calculation how many of them, are really likely to act on this suggestion, may determine whether it is wise,—and therefore, on our present assumption,—whether it is right, for any given one of them so to act.

It is here, as I have long thought, that we find the serious, and indeed the fatal defect in the merely intellectual, as contrasted with the social and sympathetic, aspect of Utilitarianism. If this were a treatise on Ethics we should have to go fully into this question, and enquire why it is that men who wish to act right very seldom do thus strike out their own line for themselves, but are constantly reckoning what would follow *if* others were to do what there are very good grounds for suspecting that they never will do. Two reasons however catch the attention at once, both springing out of our emotional or sympathetic nature. The first of these rests upon the obvious ground that we cannot help setting an example. The direct consequences of our own action may often in the long run be very small compared with the indirect consequences which follow from the actions of others who know what we do. But the firmest ground would seem to be the same as that which is always found strong enough to prevent the most consistent thinker from sinking into what may be called solitary Berkeleyanism, or in more technical language Monistic Idealism. He may try to persuade himself that he is the only sentient being in existence, and that all other men are like all other objects; that they are nothing more than groups of orderly ideas in his own mind. But his social and sympathetic instincts are much too strong for this reasonable and consistent deduction, and he soon finds himself obliged to take other sentient beings so to say into partnership, and to put them on the same footing with himself. So with our ethical schemes. Directly we attempt

secured on that day would then simply serve to swell his neighbour's profits. Again; had I been a slave merchant in the old days, I might have taken up the same position against the same argument. Whilst thoroughly admitting that the slave trade is an injury to the human race, and being perfectly prepared to give my aid to its suppression, I may urge that, pending its general suppression, my continuance in the business does no harm to any one. My action does not increase the average number of slaves imported, for by the well-known laws of supply and demand any artificial diminution of the number of traders would cause profits to rise, and would therefore attract others into the business. There are surely many earnest and sincere social reformers at the present day who justify their present attitude on this ground. They proclaim the desirability of altering our system of taxation, of changing much in our social arrangements, even of introducing some form of Communism: but they do not cease to claim all their present private rights. They hold, and few will blame them, that it does not follow that what 'ought' to be done by all collectively ought to be done by each individually.

So long as the goodness or badness of actions is estimated by their consequences, the position of such a man as we have supposed above seems impregnable, when thus defended from his own individual standpoint; and the question arises how he can ever be compelled to quit this ground, which, we must remember, is his habitual ground for judging of most of the petty actions of his daily life. Successfully to base a merely intellectual appeal to any man upon the consequences of his actions, we must surely convince him, not by tracing what would follow upon some purely imaginary supposition, but by pointing to what will actually follow from his own conduct. This points, I apprehend, to the main reason why, in such a case as that of the slave traders, we feel very little doubt in saying that the proposed ground of defence is cowardly and selfish, whereas in many other cases it is difficult not to admit that there is something to be said for such a defence. We feel instinctively that in matters which make strong appeal to our emotions and our sympathy it is idle to try to strike out a line of our own rather than to fight, so to say, side by side with the army of our human comrades.

It is on such grounds, as it seems to me, that Kant's celebrated formula for testing the rightness of an action breaks down. It is a formula of a purely intellectual character: 'Can you will the maxim according to which you are acting to be a law universal to mankind?' That there are many actions which are equally obligatory, or equally reprehensible, whether practised by one man, or by all, is unquestionable. But the attempt to carry out such a maxim in daily life would surely fail[1]. The occasions are numerous, if indeed they do not constitute the larger part of our actions, in which the wisest and most moral conduct would have to be justified from the individual point of view. The agent may doubtless steer his course in accordance with the resultant impulse of a number of more or less general maxims, but these would be modified by considerations drawn from a knowledge of what others were doing or were going to do. And one of these modifying circumstances would often consist in the knowledge that many others, similarly situated with ourselves, would certainly not take the same course. To make such a test generally suitable we should have to substitute 'end' for 'maxim'; a change, it need not be said, which would totally alter its character. If I and all others alike will the same end, we shall often find it advisable to take very different intermediate courses in order to attain this common end; and these different courses when couched in the form of maxims will require to be differently phrased.

If it were allowable to follow Bentham's example by the free introduction of new technical terms, I should be inclined to mark the above distinction by the words *autocentric* and *coinocentric*. These have, of course, no relation to the particular motive, sanction, or end which is appealed to, but simply mark the distinction between two attitudes which we may adopt in our practice. In the one case the agent is supposed, like Parnell's angel, to make himself the centre of his own speculation and consequent action; all other contemporary agents

[1] There is another objection to such a maxim, which is closely analogous to one which presented itself in the discussion on Causation. If the maxim is to be specialized so as to be throughout appropriate to the circumstances of the agent, we could not suppose any one else to have occasion to appeal to it. The action would become unique.

being classed by him among the phenomena of which he has to take account, and on which his conduct is based. In the other the agent, so to say, makes common cause with all other agents: he and they together form, for purposes of action, a kind of moral unit, and therefore look for guidance to some common maxim or rule. The former resigns to the province of Science, as distinguished from Art, every thing and person except his own individual agency. The latter makes a much larger reservation, for he regards himself as being but one member in the whole body of intelligent and moral agents. Practically, the actions of almost every man are divided in some way or other between these two distinct principles, and one of the chief ethical difficulties we experience is in deciding the partition line between them.

Political Economy, when regarded as an Art,—as we have seen that it may be regarded,—seems a case in point of a branch of conduct which furnishes scarcely any other rules than those addressed to classes of men; that is, none of which the ordinary individual agent can reasonably make use. Hence the extreme difficulty and uncertainty when any one attempts to carry out its principles into practice. Take a trivial instance. On ordinary assumptions as to competition, we gather that if people overpay cabdrivers it does not in the end result in any benefit to the drivers, because the masters will simply demand just so much more from them for the hire of a cab for the day. But, when I as an individual am dealing with my driver, I know well enough that he will be richer now by just that shilling which I am proposing to give him, and that in all probability nothing will happen in consequence to deduct a corresponding shilling from his own future earnings or from those of any other cabman. And similar considerations enter in the case of most other actions where competition comes into play. We are ready to admit that such and such consequences would follow *if* all of us acted in some assigned way, but we all proceed upon our individual paths in entire disregard of any such interposed hypothesis. The bulk of mankind will not be persuaded to class the generalizations of Economics with those which they consider to belong to ordinary Morality, or therefore to obey as individuals the rules which they would admit to be applicable to society in general. Where these generalizations do become

directly and practically effective is where the issues are in the hands of the State or of some central authority, of parliament or a powerful minister. Those whom Political Economy 'commands' or 'advises' are not the separate individuals, but the Chancellors of the Exchequer and Prime Ministers, for these can direct the conduct of the whole community.

In this connection however there is one exception which must be strongly insisted on. The distinction should be kept in view between the cases in which, as above, the appeal to the supposed generalization of the action is illegitimate, and the cases in which such an appeal is one of the appropriate applications of a Hypothesis. We considered the nature of some of these applications in a former chapter, under the head of *illustrative* hypotheses. To this class, for instance, belong the not uncommon appeals where the result of the individual action is apt to be overlooked because it seems trifling. I see some one plucking a bunch of grapes from a vineyard through which he is passing; he makes the remark that the owner will never miss it. I retort with the supposition; Well, but think what would follow if all the passers by did the same; the owner would soon be a serious loser. Now such a retort seems perfectly legitimate. The imaginary supposition is simply the single action on a large scale. By thus magnifying the consequences they are brought more plainly into view, and an obtuse or selfish mind may more easily realize them. But nothing is thus *proved*. It is merely as if we wished to convince someone that even a glass of water taken from a pond would lower its level. Should any one doubt the fact, because no eye would notice the change, we remind him that the abstraction of several hogsheads would be perceptible, and that the effect of taking the smaller quantity must be in proportion. But this is an illustration rather than a proof.

The argument against smuggling would be much more convincing if based on such a ground: that is, if the objector were to insist that he who evades the law does really do a concrete actual injury to the State, and therefore to every member of it. The introduction of the hypothetical generalization, unless it is avowedly merely by way of illustration, gives a loophole to escape. Even so cautious a reasoner as Austin seems to have tripped here. He says (*Lect. on Jurisprudence*) "If I

evade the payment of a tax imposed by a good Government, the *specific* effects of the mischievous forbearance are indisputably useful. For the money which I unduly withhold is convenient to myself; and compared with the bulk of the public revenue is a quantity too small to be missed. But the regular payment of taxes is necessary to the existence of the Government. And I, and the rest of the community, enjoy the security which it gives, because the payment of taxes is rarely evaded."

Surely this is fallacious. It is here assumed that by keeping, say, £10 to myself there is an actual balance of good, for that the gain to myself by its retention is greater than the diminution of comfort to the community by its expenditure for public purposes. The two terms of this proportion, so to say, are then equally increased; but attention is directed to the increase of one only, namely the injury resulting from loss of efficiency in the public administration, and is withdrawn from the increase of the other, namely the pleasure to the individuals by their greater power of expenditure. If the taxes of a thousand persons are thus evaded, just one thousand persons are rendered happier by having the money to spend for themselves. The sounder reply would be, By defrauding the revenue of ten pounds you *do* injure the community, and you cause a greater aggregate of injury than you cause pleasure to yourself. Taking this ground there is no objection to aiding the sluggish or selfish intellect to realize the fact by exhibiting the results on a larger scale, by aid of such a supposition as that in question. But the supposition has only an illustrative, not a probative value.

II. The next class of intrusions into the practical, or departures from the standpoint of pure Science into that of Art, is of a different kind. It arises from the impossibility of studying, or rather of publishing the results of our study, of the conduct of intelligent agents, without thereby producing a disturbance in their conduct. Any person can see that to draw inferences about a thing, and then to introduce a disturbance which was not contemplated when the inference was drawn, is to invalidate the conclusion we have obtained. But when the inference is about the conduct of human beings it is often forgotten that in the inference itself, if published, we may have

produced an unsuspected source of disturbance. In other words, if the results of our investigations be given in the form of statements as to what people are doing and what they will do, the moment these statements come before their notice the agents will be subject to a new motive, which will produce a disturbance in the conduct which had been inferred. We may make what statements and criticisms we please about the *past* conduct of men, but directly we commit ourselves to any statements about the future, or, in other words, begin to make predictions, we lay ourselves open to the difficulty just mentioned. That predictions can be made seems to be held by many of those who have adopted the application of logic now under consideration. They do not, of course, claim to be able to foretell the particular actions of individuals, but they assert that it is quite possible that we may some day be able to foretell general tendencies, and the results of the conduct of large masses of men.

The following extracts from Mill's *Logic*[1] (Bk. VI. ch. iii. § 2), will contain a good compendious description of these claims of Sociology. After referring to the condition in which astronomy once was, and in which the science of the tides now is, he describes in the following words the practical aims of Sociology, and that ideal perfection of the science from which we are precluded only by the imperfection of our faculties:—"The science of human nature is of this description. It falls far short of the standard of exactness now realized in Astronomy; but there is no reason that it should not be as much a science as Tidology is, or as Astronomy was when its calculations had only mastered the main phenomena, but not the perturbations.

"The phenomena with which this science is conversant being the thoughts, feelings, and actions of human beings, it would have attained the ideal perfection of a science if it enabled us to foretell how an individual would think, feel, or act, throughout life, with the same certainty with which astronomy enables us to predict the places and the occultations of the heavenly bodies."

[1] This particular controversy seems to remain now just about where it was then: the difficulty in question is a fatal objection to an impossible claim.

It will hardly be denied that there is the following distinct theoretical objection to the above illustration. The publication of the Nautical Almanac is not supposed to have the slightest effect upon the path of the planets, whereas the publication of any prediction about the conduct of human beings (unless it were kept out of their sight, or expressed in unintelligible language) almost certainly would have some effect. The existence of this distinction renders all such physical illustrations entirely inapplicable when we thus attempt to explain the way in which it is supposed that human conduct can be studied and foretold.

It should be clearly understood that we need not be under any apprehension of getting involved in the Fate and Free-will controversy here; the difficulty before us does not arise out of the *foreknowledge*, but out of the *foretelling*, of what the agents are going to do. Assuming that the abstract possibility of foreseeing human conduct, alluded to in the extract above quoted, is quite compatible with our practical consciousness of freedom, it must be maintained that a difficulty of an entirely distinct character is introduced the moment we suppose that this conduct is foretold, or rather, if one may use the term, *forepublished*. After all the causes have been estimated which can affect the agent, with the single exception of the sociological publication which describes his conduct, we shall very possibly find that the result is subsequently falsified by the disturbing agency of this publication itself.

This disturbance, observe, is not of the nature of a mere complication of the result; it may take the form of introducing a distinct contradiction. Some particular action was going to be performed, and was therefore announced; in consequence of the announcement that action is not performed, but something else is done instead. But had this further consequence been foreseen (as we must, on our present assumption, suppose might have been the case) and allowed for, we still shall not find any escape from the difficulty. Were this all we had to take into account we should have nothing further to apprehend than a complication; but beyond all this there is the conflict between the final announcement and the conduct announced, which cannot be avoided. It must be repeated again, that it is not foreknowledge, but foretelling, that creates the difficulty; the

observer, after he has made his announcement, or whilst he is making it, may be perfectly aware of the effect it will produce, and may even privately communicate the result to others, but once let him make it so public that it reaches the ears of those to whom it refers, and his work is undone. His position, in fact, is somewhat like that attributed to Jonah at Nineveh. Giving the prophet the fullest recognition of his power of foreseeing things as they would actually happen, we must yet admit that he labours under an incapacity of publicly announcing them in that form. The city was going to be destroyed; Jonah announces this; in consequence the people repent and are spared. But had he foretold their repentance and escape, the repentance might never have taken place. He could, of course, make a hypothetical statement, so as to provide for either alternative, but a categorical statement is always in danger of causing its own falsification.

The only reply, seemingly, can be that although the above difficulty is a theoretic objection, the effects referred to will be so nearly insignificant that they can be neglected in practice. But it is surely very doubtful whether distinct statements about human beings can be expected to produce little or no effect upon their conduct. The magnitude of this disturbing effect must depend in great part upon the nature of the particular announcement made, and the intelligence and readiness to believe of the agents referred to in it. If the announcement is concerned with matters of little importance, or with the conduct of persons who for any reason are not likely to take notice of what is said about them, then the considerations to which we have been referring might be neglected without serious error. We might calculate and publish as much as we please about the conduct and fate of the depressed classes at the bottom of the social scale, without any serious anxiety that our predictions and conclusions would in consequence be falsified, except in so far as others were interested in their fate. But we should soon find a considerable difference if we were to begin to discuss in this way the prospects of any persons who were likely to take an interest in our proceedings[1]. It may be true that at present but little effect

[1] I suspect that Political Economy supplies an actual case in point here. The more rigid school of Ricardo and his followers were fond of insisting upon

would be produced by any statements that we might publish about the future of society, because the possibility of making such statements is doubted; but if Sociology were ever to establish its claims, the effects produced in each case by its own disturbing agency would rise into real importance.

The foregoing remarks apply principally to the supposition of a prediction being distinctly falsified owing to its statements being of a disagreeable character, but it must not be supposed that the difficulty is confined to cases such as this. The prediction may be equally falsified if it holds out an attractive prospect. There will not indeed be the same direct contradiction here, arising out of the agents abstaining from what was foretold; but if, in consequence of the announcement, they perform their actions more speedily or more effectually than they would otherwise have done, the prediction is still rendered incorrect. The conduct, as it is finally carried out, is not the consequence of those motives only which had been taken into account, but of these together with the additional motives suggested by the publication. The nature of the disturbance which would be thus produced, as dependent upon the character of the announcement made and the circumstances under which it was published, have never, I think, been properly taken account of, but they seem well worthy of examination.

III. The remarks in the last few paragraphs are intended to point out that that purely speculative and isolated position of the observer, which alone is tenable when we are laying down rules for a science of inference, is one which it is in certain cases practically impossible to maintain. With every wish to be nothing more than simple observers, we cannot always secure our isolation when we are describing the conduct of intelligent human beings, for we cannot always prevent them from being influenced by what we say. The next criticism is of a very different kind. It refers not to the actual

the impotence of the labourers to influence the rate of wages by combination. Is it not likely that the stern enforcement of this doctrine may have increased the inducement of the men to rise in protest against it? I remember,—many years ago now—seeing an account of a large meeting in which one speaker declared, amidst much applause, 'They say Political Economy is against us: well, we'll be against Political Economy':—as, with unanimity, they might be, (and as they have since shown that, in certain respects, they can be).

disturbance caused unintentionally by the observer's published inferences, but to an intentional hypothetical disturbance in the actions which form the subject of the inference. The possibility of such a disturbance being contemplated arises from the fact that the observer himself, and other persons besides those to whom the inference refers, are themselves capable of acting in the same way as the persons whose conduct is described. Hence arise constant intrusions of the observer's personality into calculations from which they should be rigidly excluded. The point may seem somewhat subtle, and we must therefore bespeak the reader's attention to the following remarks, which are intended to supplement what was said in the chapter on Hypotheses.

The statistics with which we are concerned in Probability (for it is in this branch of Logic that these considerations become most prominent) are composed, in great part, of the voluntary actions of men. They may relate, for example, to crimes, such as the frequently adduced instances of murders, thefts, and suicides; to what are intended to be virtuous actions, such as the sums annually expended in charitable or other such purposes; or to actions of an indifferent character, such as the number of marriages, or of insurances effected in the year. But of such portions of human conduct, as of most other portions, it is a simple datum of experience, that in the long run, when we extend our observations over a sufficient space, a great and growing degree of uniformity is generally observable.

Now between statistics of this kind and those which are concerned with what are not the immediate results of voluntary agency, whether the latter be of a purely involuntary character, as for example shipwrecks, or be results in which the human will is generally but a remote cause, as throws of dice, or births and deaths, there is one marked difference. It is this. We the observers, or any one else whom we suppose to occupy the position of observer, are ourselves beings like those whose conduct we tabulate and reason about, and the actions in question are such as we are or may be in the habit of performing ourselves. Hence it results that we are conceivably, if one may so say, a portion of our own statistics; we may suppose our own case to be included in the statistics under discussion. In

many of the common examples taken from insurance, and above all from games of chance, the case is of course extremely different. There we may preserve with perfect consistency that purely speculative view in which we regard ourselves as looking passively on the successions of existences independent of ourselves. It would in fact be always difficult, and often impossible not to take such a view there.

But though not impossible, it is exceedingly difficult to do the same when the things whose statistics we discuss are actions which men exactly like ourselves do perform, and which we any day may perform. To retain the correct view with rigid consistency it would indeed be necessary to exclude ourselves entirely from the statistics, in other words to confine ourselves consistently to the observer's point of view, as we unavoidably do in the case of games of chance. We might help to compose the statistics of others, just as others compose the statistics for us, but we must not attempt to occupy both positions, those of observer and observed, simultaneously.

It must be admitted that owing to the peculiar character of the statistics of Probability, and the merely *average* truth with which we are there concerned, the inconsistent attempt just mentioned does not necessarily cause any error there. If indeed we were concerned with the absolute and universal statements of ordinary inference there would be error; the determination of a man, for example, to commit suicide when the inferential statement in which he was included had contemplated his abstaining from such an act, would falsify the inference. But no one man has power, as previously remarked, by his own private conduct at least, to do much injury to an average.

It is not therefore exactly by this stepping down of the observer into the arena of the statistics, unwarranted as it is, that the fallacy now to be noticed arises. It is rather by certain hypothetical intrusions to which the acknowledged practical harmlessness of the actual intrusion gives rise, that error and confusion are caused. Finding that any one observer may without mischief do very much as he likes amongst the statistics, similar invasions are conceived upon such a scale as to involve the destruction of the speculative or scientific view, and, as we shall presently see, to cause amongst other things the expression of a great deal of practical fatalism.

A quotation from Buckle's *History of Civilization* (Vol. I. p. 25) will form a convenient introduction to the discussion now to be entered upon. After pointing out that among public and registered crimes there is none which seems so completely dependent on the individual, and so little liable to interruption, as suicide, he proceeds as follows:—"These being the peculiarities of this singular crime, it is surely an astonishing fact, that all the evidence we possess respecting it points to one great conclusion, and can leave no doubt on our minds that suicide is merely the product of the general condition of society, and that the individual felon only carries into effect what is a necessary consequence of preceding circumstances. In a given state of society a certain number of persons must put an end to their own life[1]. This is the general law, and the special question as to who shall commit the crime depends of course upon special laws; which however, in their total action, must obey the large social law to which they are all subordinate. And the power of the larger law is so irresistible, that neither the love of life nor the fear of another world can avail anything towards even checking its operation."

The above passage as it stands seems a trifle absurd, and would I think, taken by itself, convey an extremely unfair opinion of its author's ability. But the views which it expresses are very prevalent, and are probably increasing with the spread of statistical information and study. They have moreover a still wider extension in the form of a vague sentiment than in that of a distinct doctrine. And as they are not now likely to find a more intelligent expositor, or to be expressed in a more vigorous and outspoken way, I cannot do better than state my opinions in the form of a criticism upon this quotation.

One portion of the quotation is plain enough. It simply asserts a statistical fact of the kind perfectly familiar to us, namely, that about 250 persons annually commit suicide in London. This is all that the statistics themselves establish. But, secondly, this datum of experience is extended by Induction. The inference is drawn that about the same number of persons will continue for the future to commit suicide. Now this, though not lying within the strict ground of the science

[1] About 250 annually, in London, at that time.

of Probability, is nevertheless a perfectly legitimate employment of Induction. The conclusion may or may not be correct as a matter of fact, but there can be no question that we are at liberty to extend our inferences beyond the strict ground of experience, and that the rules of inductive philosophy will furnish us with many directions for that purpose. We may admit therefore that, for some time to come, the annual number of suicides will in all likelihood continue to be about 250.

But it will not take much trouble to show that there is a serious fallacy involved in most cases in the expression of such sentiments as those quoted. It should be clearly understood that this fallacy finds no countenance in either of the two assumptions which are necessary for the establishment respectively of the rules of Probability and Induction, in those, namely, of statistical uniformity, and invariability of antecedence and sequence. In other words, the inference in the quotation would remain either unmeaning or false, in spite of our admitting that the number of persons who perform any assigned kind of action remains year by year about the same, and that the actions of each person are links in an invariable sequence.

We have here again forced upon our attention the distinction between the two standing-points which may be occupied when we are investigating human conduct. Let us briefly examine them in turn.

There is, firstly, that speculative point of view which, as I have said, we are in consistency bound to retain. On this view all these assertions about the inutility of efforts and the inefficiency of motives are meaningless, or rather inappropriate. What we are then discussing is, not what people might do if they were to resolve to alter their conduct, but what we have reason to infer that they *will* do. We are not concerned with the results of hypothetical alterations—these results might be of extreme importance—but we are drawing inferences as to whether such alterations will be made. All therefore that can be established by the fact of the statistical results remaining nearly the same is, that the amount of the counteracting efforts and the strength of the antagonist motives remain the same, not that these efforts and motives are in any sense ineffective. To prove this last point it would be necessary to take very

different ground, namely, to examine instances in which such efforts had been made and instances in which they had not, and to show that the results in each case were nearly or exactly the same.

In distinction from the above there is the view taken from the practical standing-point. Every agent, whether or not his conduct form part of any table of statistics, finds himself the centre of a sphere of action. This view receives immense extension by each person being able to put himself in imagination into the position of any other individual, or into that of any body of individuals. When this position is occupied the question becomes a very different one from that last considered. We are not now considering whether efforts *will* be made, but we are distinctly taking into discussion the different results according as they are made or not. This would be the most natural and appropriate explanation to be given of such remarks as those in the quotation before us, and could be the only one offered if we were referring to the efforts of a single individual or to those of a few people. All that any person could then mean by talking about the inutility of efforts would have no reference to the question whether efforts would really be made or not; he would simply mean that the difference, according as they were made or not, would be little or nothing.

It will scarcely be maintained, in this sense, that motives are feeble or efforts at suppression ineffective. Any considerable alteration in the belief of people as to a future world, or in their comfort in this world, would unquestionably have a great influence upon the number of murders or suicides. As regards the efforts of the clergy or magistrates to suppress the evil, however much these may be depreciated, it will hardly be denied that something might be done towards *increasing* the annual number of those who destroy themselves,—by removing the police, for instance, from the neighbourhood of the Serpentine and Waterloo Bridge. And it tells equally for our present argument, if it be admitted that the efforts of such persons could produce any consequence whatever, whether favourable or adverse.

The reply to this would probably be, that though considerable consequences might really follow were we to suppose an alteration in the conduct of persons in authority, or in the belief of the

people, yet that we have no right to introduce such an imaginary alteration, because we know that these causes will not really take place. This is probably quite true, and I have no intention of denying it; but what it is requisite to draw attention to, and what seems to be often overlooked, is how in such a reply as this we may be shifting from one point of view to another. We are abandoning the view taken in the last paragraphs and falling back upon the speculative view. When the efforts of a few persons are contemplated, the hypothesis of their acting otherwise is admitted, but the consequent effect is pronounced to be insignificant, as might very likely be the case. When however the efforts of many are contemplated, the hypothesis of their acting otherwise and the consequent effect, which would then be great, are not admitted, on the plea that they are inconsistent with fact.

Such a confusion as that discussed above may seem absurd, but it seems likely that in this way considerable support is often given to that practical fatalism which expresses itself in complaints about the utter impotence of the individual, and the irresistible power of great social laws, and which shows itself in our conduct by a somewhat indolent disposition to let everything good or evil take its own course without troubling ourselves about it. It is observed that in the statistics of actions which may be the result, in their final form, of many different motives and of various conflicting struggles, there is yet year by year a marked uniformity. Instead of concluding from this what alone ought to be concluded from the standing-point of a science of inference, namely that the motives and the efforts remain about the same year by year, a confusion is made between this and the practical view, and the doctrine is deduced that the efforts at repressing such conduct are unavailing.

Such fatalistic views are often expressed in the form of disparaging comments upon the insignificance of individual efforts. In the sense in which this complaint is often made, it is surely little more than an expression of our own indolence or selfishness, and really means, not that the results we could effect are small, but that we care little about them. Test this by taking some statistical uniformity, in which the motives that act to produce the result are almost entirely of a self-regarding character. Suppose that any one having ascertained that

about a thousand persons, daily, in London, who could afford to eat a dinner, do for one reason or another go without it, were to announce this fact as a social law of much efficacy, and were to declare that its power was such, when examined on a large scale, that neither the fear of future hunger nor the love of good food could prevail towards even checking its operation, what would be the natural reply? The form of these statistics and the nature of the argument grounded upon them are very nearly identical with those of the example cited by Buckle. The reply would probably be, that if we were professing simply to draw inferences, most of this talk about the impossibility of checking such actions was, to say the least, inappropriate; but that if we were taking the practical view, that is, if we were for any purpose putting ourselves into the position of one or more of the individuals in question, the statement was utterly false. Each one of those men could in most cases have eaten his dinner or not according as he pleased, and therefore the whole body could have done so under the same conditions. And no sophistry about free-will and necessity would be allowed for a moment to stand in the way of such a judgment. Equally absurd would it be to talk about the insignificance of individual efforts in the face of such a great social law. When therefore it is vaguely complained that efforts are fruitless in the case of crimes, is there not some ground to think that the real meaning is that such efforts, on any much larger scale, are not likely to be made? And when it is urged that the individual can do nothing in this case, whilst no one would dream of making the same assertion in the other case, are we wrong in thinking that the real difference is that the attainment of one's own dinner is more universally regarded as a substantial good than the suppression or diminution of our neighbour's faults? It cannot, of course, be denied that we should find it much more easy to dissuade people from some courses of conduct than from others; all that is meant is, that there is no real distinction between the general deductions which may be drawn from one or another of these various kinds of statistical regularity.

Again, in the opposite direction to that of this practical fatalism, it is not possible to read far in most speculations on social matters without seeing cause to wish that the writer

had been more precise in indicating what exactly he proposed to leave undisturbed, and what was included in the scope of his suggested alterations and improvements. As this vagueness has been already discussed in the Chapter on Hypotheses little more need be added here. The inventors of Socialistic schemes for the entire reconstitution of Society are perhaps hardly to be taken seriously. Their elaborate and complex contrivances could not be introduced in the 'bit by bit' fashion. They would demand for their success an almost simultaneous effort on the part of a working majority of society, with some corresponding compliance on the part of the remainder; contingencies which may be left aside as mere dreams. In fact the scheme of a Fourier may fairly be classed with those of a More or Harrington: the distinction that the former writer possibly did believe that his plan would some day be adopted, whilst the latter did not suppose that theirs ever had existed, is, for all but themselves, insignificant. The only value of all such schemes alike, beyond the gratification felt in contemplating an ingenious work of art, is to be sought in the casual details of suggestion which may be gathered from them. But it is not only in the case of these elaborate schemes that such reflections arise. Who can read the recommendations which are urged on every hand by social reformers, without serious misgivings as to the significance of the hypotheses which are introduced? 'If only the clergy,—or the squires, or whoever else it may be, —would but arouse themselves in real earnest, what a change one generation would witness in the condition of the labourers!' True, but is it not equally easy to wish or imagine such an end as such a means; and, when we are dealing with whole bodies of men, is the one really more likely of introduction than the other? But as this aspect of the subject has been sufficiently discussed in a former chapter we need not dwell upon it further here.

The reader will recognize that the remarks made in this chapter amount to nothing more than taking a step or two upon a long and intricate path. They are, so far as this work is concerned, the final repetition and enforcement, as a general conclusion, of what was indicated as a postulate in the first chapter. The more incisively we enuntiate and claim the passively inferential attitude of pure Science, the more distinct

becomes the contrast, and yet the more intimate the connexion, between this and the actively practical attitude of Art or Conduct. It is not only that the latter has to be founded upon the former and therefore to advance with it. The interdependence is much closer and more intricate than this. If indeed Science dealt only with material objects, and Art only with voluntary human actions, most of the problems here suggested would either never have been raised or would be lightly solved. It is when Science becomes more comprehensive, and claims for itself the right to observe and to judge and to infer human actions in precisely the same general way as if they were mere physical events, that dispute and confusion begin to arise. How indeed could this fail to be so, when each one of us thus becomes an almost involuntary and passive portion of the data of Science which lie about another man as observer, whilst he remains a centre of conscious voluntary activity in himself? He is not only engaged in observing the similar materials grouped about *him*, but he is scheming to modify their course both for his own private ends and for those of Society as well.

It may sound frivolous to suggest that there is an opening here for a new Science, and presumptuous to suggest that if such an opening were admitted we might invent the name *Practic* for it, in analogy to Logic. But, after all, such a suggestion implies nothing more than that the inevitable advance of speculation has brought into greater prominence a set of topics which, though they had certainly not been entirely overlooked, had nevertheless been unduly neglected. Were such topics specialized into a new group, they would gain at once by the consequent concentration of study upon them, and by the light which they mutually cast upon each other.

What is intended by the above mention of an analogy to Logic is this. Logic, investigating all the various sciences, makes abstraction of what may be called the forms of them, that is, of certain elements which are common to them all. By such abstraction we obtain certain processes, such as those of induction and deduction, or the more elementary processes into which these may admit of being analysed; and certain materials or results, such as terms, propositions, definitions, and so forth. The elements thus common to all the sciences and special to none, make up what we call Logic; at any rate they comprise

the bulk of what is here understood by Material or Inductive Logic. Now conceive the same kind of abstraction applied to all the various Arts, and we should similarly obtain certain perfectly general results and processes. Of the former we may give, as an instance, *Rules of Action*, which are, so to say, the principal materials with which the Arts are built up. Of the latter, again, there is an instance in *Hypotheses*, without resort to which, as we have seen, scarcely a step can be taken in Practice or Conduct.

Whether or not a new name be assigned, and whether the subject matter itself be relegated to Logic or to Ethics or be discussed apart, the main topics with which we should be thus concerned may be readily indicated. They may be summed up by saying that they comprise the pure Theory of Rules of Action, and of the processes of applying such rules in Conduct generally. What, for instance, is the nature and function of a Rule of Action, considered apart from all reference to the ultimate End at which it is aimed, and to the motives which incite us to follow it? What are the principal subdivisions of such rules, and are they all alike truly general rules? Are they equally relevant to the individual agent and to the class to which he is assigned? Can any test be furnished to decide when appeal should be made to a rule, and when the agent should act as a solitary individual? And as regards the resort to Hypotheses; what are their functions? And what the limits of their admissibility when introduced into speculations upon social phenomena? Such questions as these must always, as occasional problems, have occupied the attention of the thinker; and also, as practical difficulties, have impeded the path of the humblest agent. But their interest and importance appear to be on the increase. For as the range of Science is enlarged, and its accuracy and certainty are improved, the more numerous are the occasions on which rational conduct must appeal to its conclusions, and the more precise the answers which we expect it to furnish.

It is in Ethics that such topics have hitherto chiefly claimed attention, but their scope is far wider than the limits conventionally assigned to Ethics; being in fact co-extensive with Conduct generally. Precisely similar difficulties recur in the applications of every science which deals with human conduct;

prominently, for instance, in those of Political Economy, of Jurisprudence, of Statistics, of Sociology, and in the speculations of socialists and reformers in general. In every one of these studies we meet with indications, so to say, of a missing science. This is not so much shown by the absence of agreement about facts of detail, for plain men will always find that they disagree here; nor by disputes about ultimate principles, for thinkers will never abandon their immemorial rights there; but rather in the general lack of any commonly recognized body of axiomata media, or middle propositions. Within this limited range it does not seem unreasonable to hope that the majority of human minds, which after all much resemble each other, should in course of time come into fairly close agreement. The remarks in this chapter are designed to indicate the principal directions in which this agreement is still lacking, and to furnish a small contribution towards its ultimate attainment.

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CAMBRIDGE: PRINTED BY JOHN CLAY, M.A. AT THE UNIVERSITY PRESS.